PAPERS AND PROCEEDINGS

OF THE

Seventy-third Annual Meeting

OF THE

AMERICAN ECONOMIC ASSOCIATION

St. Louis, Missouri, December 28-30, 1960

Edited by JAMES WASHINGTON BELL, *Secretary of the Association*

and

GERTRUDE TAIT, *Executive Assistant*

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PROGRAM OF THE SEVENTY-THIRD ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

St. Louis, Missouri, December 28-30, 1960

The sessions are organized around the broad theme, "Frontiers of Economic Knowledge."

Tuesday, December 27, 1960
6:00 P.M.

Executive Committee Dinner Meeting

Wednesday, December 28, 1960
9:30 A.M.

Monetary Theory: New and Old Looks
  Chairman: ELI SHAPIRO, Massachusetts Institute of Technology
  Papers: JAMES TOBIN, Yale University; JOHN KAREKEN, University of Minnesota; KARL BRUNNER, University of California, Los Angeles
  Discussants: E. S. SHAW,[1] Stanford University; THOMAS MAYER, Michigan State University; RICHARD A. MUSGRAVE, Johns Hopkins University
Antitrust Problems
  Chairman: EDWARD S. MASON, Harvard University
  Papers: M. A. ADELMAN, Massachusetts Institute of Technology; ALMARIN PHILLIPS, University of Virginia; DONALD J. DEWEY, Columbia University
  Discussants: JAMES W. McKIE, Vanderbilt University; REUBEN E. SLESINGER, University of Pittsburgh; JEROME B. COHEN, College of the City of New York

2:30 P.M.

Public Utilities and Transportation
  Chairman: BEN W. LEWIS, Oberlin College
  Papers: JAMES C. BONBRIGHT, Columbia University; JOHN R. MEYER and GERALD KRAFT, Harvard University
  Discussants: H. THOMAS KOPLIN, University of Oregon; DANIEL MARX, JR., Dartmouth College; RICHARD A. TYBOUT, Ohio State University
Frontiers in Uncertainty Theory: The Evidence of Futures Markets (Joint session with the Econometric Society)
  Chairman: D. GALE JOHNSON, University of Chicago
  Papers: HOLBROOK WORKING, Stanford University; HENDRIK S. HOUTHAKKER, Harvard University; PAUL COOTNER, Massachusetts Institute of Technology
  Discussants: RUTH P. MACK, New York, N.Y.; MICHAEL BRENNAN, Brown University; MARC NERLOVE, Stanford University
Economic Analysis of Urban Problems (Joint session with the Regional Science Association)
  Chairman: SHERMAN J. MAISEL, University of California
  Papers: CHARLES M. TIEBOUT, University of California; BENJAMIN CHINITZ, University of Pittsburgh; LOUIS WINNICK, New York State Commission on Economic Expansion
  Discussants: BARBARA R. BERMAN, Harvard University; BRITTON HARRIS, University of Pennsylvania; IRVING MORRISSETT, Purdue University

8:00 P.M.

Presidential Address[2]
  Chairman: PAUL H. DOUGLAS, United States Senate
  Address: THEODORE W. SCHULTZ, University of Chicago

Thursday, December 29, 1960
9:30 A.M.

Economic Development in Mainland China
  Chairman: FRANKLIN L. HO, Columbia University
  Papers: TA-CHUNG LIU, Cornell University, and K. C. YEH, The RAND Corporation; CHOH-MING LI, University of California; ALEXANDER ECKSTEIN, University of Rochester

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[1] No Manuscript received.
[2] Published in the March, 1961, issue of the *American Economic Review*.

Discussants: FRANKLYN D. HOLZMAN, University of Washington; SIDNEY KLEIN, Rut
gers University; JOSEPH S. BERLINER, Syracuse University
Macroeconomic Theories of Income Distribution
Chairman: MARTIN BRONFENBRENNER, University of Minnesota
Papers: MELVIN W. REDER,[2] Stanford University; SIDNEY WEINTRAUB, University of
Pennsylvania; OSWALD H. BROWNLEE, University of Minnesota, and ALFRED H. CON-
RAD, Harvard University
Discussants: ROBERT W. OZANNE, University of Wisconsin; BORIS P. PESEK, Michigan
State University; ALLAN M. CARTTER,[2] Duke University
Capital Theory
Chairman: ROBERT DORFMAN, Harvard University
Papers: OTTO ECKSTEIN, Harvard University; JOHN W. KENDRICK, George Washing-
ton University; JACK HIRSHLEIFER, The RAND Corporation
Discussants: FRANCIS M. BATOR, Massachusetts Institute of Technology; VERNON L.
SMITH, Purdue University; ZVI GRILICHES, University of Chicago

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Joint Luncheon with the American Finance Association[4]
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Speaker: THEODORE O. YNTEMA, Ford Motor Company
Economic Education: Challenge to Our Profession
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Papers: PAUL R. OLSON, State University of Iowa; HOWARD S. ELLIS, University of
California, Berkeley; G. L. BACH, Carnegie Institute of Technology
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University; C. ADDISON HICKMAN, Southern Illinois University
Problems of Economic Instability in Other Countries
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Papers: ERIK LUNDBERG, University of California, Berkeley; GEORGE H. HILDEBRAND,
Cornell University; SHIGETO TSURU, Hitotsubashi University
Discussants: GARDNER ACKLEY, University of Michigan; L. M. KOYCK, Netherlands
Economic Institute, Rotterdam

8:00 P.M.

Invited Lecture
Chairman: LLOYD G. REYNOLDS, Yale University
Paper: HARRY G. JOHNSON, University of Chicago
Discussants: ALVIN H. HANSEN,[2] Harvard University; DAVID McCORD WRIGHT, McGill
University; ABBA P. LERNER, Michigan State University; LAWRENCE R. KLEIN,
University of Pennsylvania
Soviet Wage Structure (Joint session with the Industrial Relations Research Association)[5]
Chairman: ABRAM BERGSON, Harvard University
Papers: WALTER GALENSON, University of California, Berkeley; GARDINER CLARK, Cor-
nell Universtiy
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Statistics

Friday, December 30, 1960
9:30 A.M.

The Balance of Payments of the United States: Problems and Prospects
Chairman: CHANDLER MORSE, Cornell University
Papers: HAL B. LARY, National Bureau of Economic Research; J. HERBERT FURTH,
Board of Governors of the Federal Reserve System; E. M. BERNSTEIN, E.M.B., Ltd.

[2] To be published elsewhere.
[4] To be published by the American Finance Association.
[5] To be published by the Industrial Relations Research Association.

Discussants: JAMES BURTLE, W. R. Grace and Company; PETER B. KENEN, Columbia University; JAROSLAV VANEK, Harvard University

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Papers: EMILE BENOIT, Columbia University; DONALD V. T. BEAR, Stanford University, and PAUL G. CLARK, Williams College; THORNTON READ, Bell Telephone Laboratories; DANIEL ELLSBERG, The RAND Corporation; ALLEN R. FERGUSON, The RAND Corporation

Distribution Costs: Concepts and Measures (Joint session with the American Marketing Association)
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Discussants: WARREN U. BILKEY, University of Notre Dame; REAVIS COX, University of Pennsylvania

2:30 P.M.

The Influence of Moral and Social Responsibility on Economic Behavior
Chairman: LELAND J. GORDON, Denison University
Papers: COLSTON E. WARNE, Amherst College; ERNEST DALE, Cornell University; ARCH TROELSTRUP, Stephens College
Discussants: HOWARD M. TEAF, JR., Haverford College; RAYMOND T. BYE, University of Pennsylvania; DEXTER M. KEEZER, McGraw-Hill Publishing Company

The Case of India
Chairman: WILFRED MALENBAUM, University of Pennsylvania
Papers: GEORGE ROSEN, United Nations Secretariat; DON D. HUMPHREY, Fletcher School of Law and Diplomacy; ASHOK MITRA, International Bank for Reconstruction and Development
Discussants: WILLIAM W. HOLLISTER, Washington, D.C.; CHARLES WOLF, JR., The RAND Corporation

Managerial Economics: A New Frontier?
Chairman: MERTON H. MILLER, Carnegie Institute of Technology
Papers: W. W. COOPER, Carnegie Institute of Technology; WILLIAM J. BAUMOL, Princeton University; CHARLES J. HITCH, The RAND Corporation
Discussants: JULIUS MARGOLIS, University of California, Berkeley; ALAN MANNE,' Yale University; FRANCO MODIGLIANI, Northwestern University

5:00 P.M.
Business Meeting

6:00 P.M.
Executive Committee Dinner Meeting

INVITED LECTURE

THE *GENERAL THEORY* AFTER TWENTY-FIVE YEARS

By HARRY G. JOHNSON
University of Chicago

What should one say in an address commemorating the twenty-fifth anniversary of a book? Normally, one thinks of the publication of a book as a birth of new ideas; and the appropriate occasion for a speech is either the day the youth attains his majority, when he is congratulated on the achievement of adult status and a brilliant future is predicted for him, or the day the old man retires, when he is congratulated on a lifetime of productive labor and wished a peaceful old age. A twenty-fifth anniversary suggests a marriage—a union of ideas with literary expression, so to speak—and the appropriate speech is one which compliments the couple on their success in solving the problem of marital adjustment and congratulates them on the number and promise of their progeny, while tactfully refraining from mentioning that in its early years the marriage was judged by many to be a mistake and doomed to failure. It is in this spirit that I wish to commemorate the silver anniversary of the *General Theory* in this lecture. We have had the coming-of-age party in the revolutionary days of the late thirties and the pension presentation ceremony in the obituary assessments of the late forties. We are now well into the post-Keynesian era; and it seems more appropriate at this point to take stock of the intellectual capital embodied in the *General Theory* than to debate whether the investment should be considered the foundation of our fortune or written off as a dead loss.

To keep the subject within bounds, I propose to concentrate on the *General Theory* and to use it as a basis for discussion. I shall first discuss the book as economic literature. My excuse for so doing is that the book long ago attained the status of a classic—meaning a book that everyone has heard of and no one has read—and the translation of its untidy construction into neat models suitable for geometric and mathematical manipulation, effective as it proved in converting economists to the Keynesian school, has obscured some characteristics of Keynes's methods of analysis which are relevant to the evaluation of his theory. From the book I shall proceed to the theory presented in it, considering it first as monetary theory and then as a theory of income and employment. I shall then comment, more briefly, on the policy implications of

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the theory. In each case I shall be concerned to assess Keynes's ideas in the context of subsequent developments; and I shall conclude with a summary assessment of the contributions of the *General Theory* to modern economics.

I. *The* General Theory *as Economic Literature*

Not even the most ardent admirer of Keynes's powers as an expositor of economic ideas and a literary stylist would wish to have his reputation in these respects judged solely by the *General Theory*. Arresting phrases and brilliant passages there are, as everyone who has read it remembers, but the book as a whole is not easy to read and master, and it has not become easier with the passage of time In support of this judgment one need only refer to the fact that no less than three successful books aimed at guiding the reader through the *General Theory* have appeared at intervals in the past twenty-five years—those of Joan Robinson, Dudley Dillard, and Alvin Hansen—not to speak of less well-known monographs and the countless interpretative articles which still continue to appear.

For the difficulty of the *General Theory* there are a variety of reasons. At the literary and expository level, there is the evident strain of the "long struggle of escape . . . from habitual modes of thought and expression" mentioned by Keynes in his Preface. There is also the non-rigorous Cambridge style of theorizing, the didactic Marshallian style, in which awkward complications are hidden in plain view and common sense is allowed to run away with the argument—a style which Keynes defended in his critical remarks on mathematical economics. There is the intrusiveness of Keynes the philosopher, interrupting the argument to muse on the social virtues and vices of the organized speculative markets on which he had made his own and his College's fortune, or to dilate rather pretentiously on the essential properties of interest and money. And above all there is the pervading influence of Keynes the born propagandist, with his instinct for dramatizing his ideas, and his Cassandra complex, fortified as polemicists often are by a certain obtuseness in understanding the arguments of his adversaries.

In directing his attack at the neoclassical concept of an economic system equilibrating at full employment and presenting a general theory of underemployment equilibrium of which the neoclassical theory was a special case, Keynes's polemical instinct was surely right, both because neoclassical ways of thinking were then a major obstacle to sensible antidepression policy and because, for professional economists, the concept of equilibrium has always had far more intellectual sex appeal as an analytical companion than its opposite, disequilibrium. But his concentration on equilibrium was in the longer run inimical to

his purpose, since his central theoretical contribution—that in a monetary economy the stability of employment in the face of changes in aggregate demand for output depends on the uncertain monetary effects of changes in money wage levels, which changes may themselves be slow—could be and was easily converted into a demonstration that his underemployment equilibrium depended on wage rigidity or on special empirical assumptions about the monetary consequences of wage changes. I shall return to this point later; for the present, let me merely remark that the polemical spirit impedes the argument.

These are idiosyncrasies of method of presentation; more fundamental difficulties for the reader are inherent in the analytical content of the book. In the first place, it is difficult for a modern reader to appreciate, after twenty-five years of rapid theoretical development, the extreme limitations of the concepts then available for dealing with economic aggregates and economic dynamics. Keynes tells us that "the three perplexities which most impeded my progress in writing this book, so that I could not express myself conveniently until I had found some solution for them, are: firstly, the choice of units of quantity appropriate to the problems of the economic system as a whole; secondly, the part played by expectations in economic analysis; and thirdly, the definition of income." These perplexities reflect the absence, in the Marshallian partial-equilibrium tradition, of a clear notion of real income; the lack of a technique of dynamic analysis in the Hicksian sense, explicitly incorporating expectations; and the fact that national income accounting was in its infancy. In resolving these perplexities, Keynes was thrown strongly back on the very classical tradition he was seeking to attack. His choice of the wage unit depended on the extremely questionable classical view that labor is a uniquely homogeneous aggregate. His treatment of expectations in terms of states of expectation, and especially his distinction between short-term and long-term expectations, incorporated the pseudo-dynamics of the Marshallian distinction between short-period and long-period analysis. And his laborious discussion of the definition of income was essentially an elaboration of the Marshallian short-period theory of the firm. More fundamentally, the theory of the book is constructed on the model of Marshallian short-period equilibrium; it incorporates the same assumptions of fixity of capital stock and increasing costs and the same vagueness as to the time-period for which the analysis is relevant. This vagueness is an especially serious weakness in the *General Theory,* which attempts to bring markets with widely different speeds of adjustment—the goods market, the money market, and the labor market—into one short-period equilibrium analysis; and much of the subsequent criticism of the *General Theory* is essentially an iteration of the inade-

quacies of Marshallian short-period equilibrium as a technique of aggregative dynamic analysis.

A second source of difficulty for the reader is the fact that, as Keynes noted in his Preface, the ideas of the *General Theory* evolved from those presented in the *Treatise on Money*. The *Treatise* received rather rough treatment from its critics, and the tendency among both Keynesians and anti-Keynesians has been to forget it. But the *Treatise* contains important clues to Keynes's methods of reasoning, and also an extensive analysis of various problems which he consequently dealt with only sketchily in the *General Theory*. The presence of the *Treatise* in the background accounts for the gap left in the *General Theory* between the marginal efficiency of capital, as the prospective rate of return on new investment goods, and the pure rate of interest, as determined by liquidity preference and the quantity of money—a gap very inadequately bridged by the concepts of lender's and borrower's risk and the later addition of the concept of finance. It also accounts for the rather cursory treatment in the *General Theory* of the theory and practice of monetary policy—a subject dealt with at length in the *Treatise*.

A third source of difficulty is Keynes's clumsy and misleading way of presenting what is essentially a general equilibrium model as a system of unidirectional causation. I refer to the order of analysis of the *General Theory*, in which income is defined as the sum of consumption and investment; consumption is determined by investment through the multiplier; investment is determined by the marginal efficiency of capital and the rate of interest; and the rate of interest is determined by liquidity preference and the quantity of money; but at the very last stage of the argument the level of income re-enters as a determinant of liquidity preference, so that the apparently simple line of causation from the demand for and supply of money to the interest rate to investment to consumption to income vanishes completely. Keynes's method, carried over from the *Treatise*, of placing the saving-investment relation in the center of the picture and working backwards to the demand and supply of money is admirably adapted to concentrating attention on effective demand and diverting it from monetary complications; but it is also apt to be misleading. It misled Keynes himself into important errors of statement, of which the most serious is the doctrine that the rate of interest is nothing more than the price for surrendering liquidity; and it has misled both critics and disciples into numerous misinterpretations of Keynes's theory. At one time it even led so great an authority as Alvin Hansen into the belief that Keynes's interest theory is indeterminate.

A fourth difficulty, especially troublesome to anyone who comes to

the *General Theory* after being introduced to Keynesian ideas at the textbook level, arises from Keynes's attempt to apply the Marshallian short-period model to the analysis of an economy in which behavior is governed by expectations about the future. There are really two theories in the *General Theory*. One is the theory that, given the money wage rate, the equilibrium levels of aggregate income and the rate of interest are determined by the propensity to consume, the investment demand schedule, liquidity preference, and the quantity of money, the first three being stable functions in the short run. The other, which finds its fullest expression in the "Notes on the Trade Cycle," is that all three of the fundamental propensities are highly unstable, under the influence of changing expectations about the future. It is the first theory, of course, which has become *the* Keynesian theory—inevitably so because it alone offers a system of relationships amenable to theoretical manipulation and empirical application. The second theory, carried to its logical extreme, would amount to a negation of the first theory. This was clearly not Keynes's intention; but he offered no formal analysis of the formation of expectations, and it was left to later writers to develop his short-period analysis into theoretical and econometric models of cycles and growth.

I have dwelt on the difficulty of the *General Theory* as a book, not with the intention of leading up to the remark that in the thirties the effort required to open the oyster led those who were successful to overvalue the intellectual pearl within—which is true but trite—but to emphasize the necessity of distinguishing between the *General Theory* as one of the great books in our literature and the general theory as a system of analysis, and to enable me to place the book conveniently in its historical setting. In so doing, I have stressed the extremely Marshallian character of Keynes's theory, which I regard not as a qualification of his achievement but as a measure of the limitations which his powers of original thinking enabled him to transcend. The *General Theory* is built on Marshallian concepts. In the light of subsequent developments, it is also possible to detect Marshallian influences at a more subtle level, in Keynes's concentration on the propensity to consume. His emphasis on personal saving behavior to the neglect of corporate saving behavior reflects Marshall's inability to integrate the modern corporation into his system of economic analysis. More fundamentally, his stress on current income receipts as the prime determinant of current consumption expenditure, and particularly his deduction of the form of the income-consumption relationship from an a priori "fundamental psychological law," reflects the general weakness of the Cambridge School in dealing with capital in its relation to economic behavior. Indeed, if one seeks for a single peg on which to hang

a discussion of both the criticism and the elaboration of this and other aspects of Keynes's theory, one can find it in the inadequate attention paid in the *General Theory* to problems of capital theory.

II. *The* General Theory *as Economic Theory*

Let me now turn from the *General Theory* as economic literature to the general theory as economic analysis. The theory presented in the book can be considered from two points of view. As Keynes presented it, it is a theory of the determination of income and employment, in which the emphasis is thrown on the determinants of effective demand and monetary factors play a subsidiary role. But it can also be considered as a monetary theory, in which the emphasis is thrown on the demand for money as an asset alternative to other assets. The distinguishing feature of the first theory is the concept of the propensity to consume; the distinguishing feature of the second is the speculative demand for money—"liquidity preference proper." Both theories are developed on the assumption—to which Keynes did not always consistently adhere—of a given level of money wages, an assumption incorporated in his device of measuring all aggregates in wage units. It is this assumption which makes Keynes's monetary theory a theory of interest rather than of prices and which raises the central theoretical questions about both his monetary theory and his theory of underemployment equilibrium.

Before I discuss these theories in detail, let me note briefly that Keynes's way of presenting them as static equilibrium theories led in each case to a violent and prolonged controversy which has turned out in retrospect to be sterile. I refer to the controversy over the savings-investment identity, and the loanable funds versus liquidity preference, stocks versus flows, debate. The savings-investment controversy turns on the question of how savings and investment can be identical and yet their equality be a condition of income equilibrium; the answer is simply that the term "savings" (or "investment") is being used in two senses—variously distinguished as *ex post* and *ex ante*, realized and intended, actual and desired—of which the latter is the theoretically relevant one. Most theorists have long since ceased to worry about the necessary identity of savings and investment, since insistence on it clutters up dynamic analysis; but it survives as a bewildering element in various Keynesian theories of income distribution, and, in the form of the proposition that investment creates its own savings, continues to inhibit clear thinking on problems of promoting economic development.

The liquidity preference-loanable funds debate turns on the question of whether the rate of interest is better regarded as equilibrating the

flow of funds onto and off the market for securities or as equilibrating the demand for and supply of the stock of cash. The answer, which is now so deeply embedded in mathematical argument that no one can be sure he has got it right, seems to be that the stock-flow distinction is irrelevant, since either theory can be expressed in stock or flow terms; and that it makes no difference whether one works with money or securities, provided, first, that one is concerned only with the determination of the equilibrium level of the rate of interest, and, second, that one realizes that this is a general equilibrium problem which can be reduced only by artifice to a problem of equilibrium in one market. In more formal terms, if one assumes to begin with that the markets for goods and factors are in equilibrium, equality between the demand for and supply of money implies equality between the (stock and flow) demand for and supply of loans and vice versa. The two theories become different, however, when applied to dynamic analysis of disequilibrium situations, since liquidity preference theory implies that the rate of interest rises only in response to an excess of the demand for over the supply of money, whereas loanable funds theory implies that it rises only in response to an excess of supply of over demand for securities, and when the goods and factors markets are out of equilibrium an excess demand for money does not necessarily imply an excess supply of securities. In a dynamic context, the loanable funds theory definitely makes more economic sense; and the sustained resistance of Keynesians to admitting it, evident most notably in the prolonged defense in the English literature of the proposition that an increase in the propensity to save lowers the interest rate only by reducing the level of income, is a credit to their ingenuity rather than their scientific spirit.

1. *The* General Theory *as Monetary Theory.* With these preliminaries out of the way, I turn first to Keynes's general theory considered as a monetary theory. As I have stated, the distinguishing feature of this theory is the emphasis placed on the demand for money as an asset alternative to other yield-bearing assets rather than as a medium of exchange, together with the crucial role assigned to uncertainty of expectations about future interest rates in determining the shape of the demand curve for money as a function of the rate of interest. Thus Keynes made the analysis of the demand for money explicitly a branch of capital theory, whereas the role of money as a form of wealth-holding had been left implicit in the neoclassical analysis. His theory of the demand for money is, however, misleadingly presented, very confused, and, as a theory of demand for money in capital theory terms, seriously incomplete; so that much work has been required of interpreters and critics by way of clarifying and extending his central ideas.

As to misleading presentation, I have already made the point that Keynes's presentation of a general equilibrium system as one of unidirectional causation creates the false impression, of which Keynes himself was the chief victim, that holding securities is the only relevant alternative to holding money, and that the classical alternatives of spending money on consumption or investment play no part in determining the demand for money and the rate of interest.

As to confusion, I need only mention the transmogrification of the precautionary and speculative motives for holding money between the two chapters in which they are discussed. The precautionary motive starts as the senior partner, entrusted with the important business of avoiding uncertainty about future rates of interest, while the speculative motive is the junior partner who looks after the possibility of profiting from a fall in security prices on an organized market. But when next we meet them, the speculative motive has taken over the whole business of asset management, and the precautionary motive has been reduced to a poor relation eking out his existence in the household of transactions demand. That the speculative motive in Keynes's final formulation of it includes the precautionary is not generally recognized in the post-Keynesian literature, even though the precautionary motive provides the ultimate rationale of the "liquidity trap." James Tobin's important article on "Liquidity Preference as Behavior Towards Risk" does distinguish clearly between the two elements in Keynes's analysis of the demand for money as an asset; but it does not identify them with the two motives that Keynes described.

As to incompleteness, there is first the fact that Keynes dealt only cursorily with transactions demand, which he explained on classical lines of personal convenience and economic structure and took to be a simple proportion of income. Thus demand for money in his theory depends partly on income, in a way not rigorously analyzed, and partly on the influence of current and expected interest rates on the preferred disposition of wealth. Subsequently Baumol and Tobin have shown how transactions demand can be treated as a problem in capital (specifically, inventory) theory, and that the demand so derived varies inversely with the rate of interest and is subject to economies of scale.

Far more serious, however, are the limitations resulting from Keynes's procedure of conducting his analysis on the assumption of a given wage/price level and lumping all securities together in an aggregate yielding a single rate of interest. Aggregation undoubtedly tends to exaggerate the importance of the speculative, as distinct from the precautionary, demand for money, since it overlooks the likelihood that, with a wide variety of equities and fixed-interest securities of varying maturity available, speculation will take the form of movements be-

tween securities of different types rather than between securities and cash. It is Keynes's emphasis on the speculative as against the precautionary motive that more orthodox monetary theorists have tended to find objectionable. The assumption of a given wage/price level excludes the influence of price expectations on the assets demand for money, and the associated necessity of distinguishing between real and nominal interest and between fixed-interest-bearing securities and equities; Keynes's attempt to circumnavigate these complications by confining the influence of price-level expectations to the marginal efficiency of capital is not convincing.

The theory of liquidity preference has since been extended by Keynesian writers—Joan Robinson, Richard Kahn, and others—to comprise choices between money, bills, bonds, and equities, and more generally between a multiplicity of assets; but the influence of price-level expectations on asset choices has generally been neglected by Keynesian writers, important though that influence has become on investor behavior since the war. For explicit analysis of it one must turn to the modern quantity theory literature, where Milton Friedman's restatement of the quantity theory of money goes far towards providing a synthesis of Keynesian and classical approaches to the demand for money in capital theory terms.

As a theory of the demand for money as an asset, Keynes's liquidity preference theory is incomplete in another significant respect. His concern with the short run in which the stock of physical capital is given, together with his assumption of a given wage level, enabled him to develop the demand for money as a function of current and expected interest rates without explicitly introducing the value of assets. This omission led him into analytical errors of far-reaching significance. In the first place, he did not distinguish between increases in the quantity of money resulting from gold discoveries or budget deficits and those resulting from open market operations, though the former involve a net increase in the quantity of assets held by the public and the latter do not. Secondly, in treating the effect of a reduction in money wages as a reduction in the transactions demand for money—a procedure which incidentally commits the heinous crime of building money illusion into the assets demand for money—he overlooked the effect of the wage reduction in increasing the public's real wealth—the Pigou-Haberler-Patinkin wealth effect. In each case, the significance of the oversight lies less in its implications for the demand for money than in the neglect of the effects of the increase in real wealth on aggregate demand. Keynes's neglect of the wealth-effect of deficit finance undoubtedly contributed to misunderstanding of the postwar consequences of the methods adopted for financing the war; and the wealth-effect of wage re-

duction has become the foundation of the proof that his underemployment equilibrium depends on rigid wages. The value of wealth and its dependence on the price level have since been integrated with other determinants of behavior in Keynesian theory.

So far I have been discussing Keynes's theory of the demand for money and subsequent developments of it. I have taken his central contribution to be his conception of money as an asset whose usefulness springs from uncertainty about future asset prices and the chief limitation of his analysis to be his concentration on expectations of future changes in interest rates as the determinant of the assets-demand for money. I now turn to the deeper issues raised by Keynes's treatment of the demand for and supply of money as determining the rate of interest rather than the level of prices. As Modigliani showed in his classic *Econometrica* article, this treatment depends on Keynes's assumption of rigid wages: with perfect wage and price flexibility, liquidity preference and the nominal quantity of money would determine the level of prices and not the rate of interest in Keynes's model, unless a liquidity trap intervenes. But an economy with perfect price flexibility is not the economy with which Keynes was concerned, and it is not an interesting economy to posit for theorizing about the problems with which he wanted to deal. In this judgment I derive support from the fact that the quantity theory has ceased to be a theory of prices and has become the theory that there is a stable demand function for money—a formulation which leaves open the question whether the demand and supply of money determine interest or prices or both.

To leave the matter there, however, is to confine the issue to short-run analysis, whereas it goes much deeper. The fundamental contention of Keynesian monetary theory is that a monetary economy is essentially different from a barter economy—that money is not merely a veil but exercises an influence of its own in the working of the economy. To examine the validity of this contention it is necessary to investigate the role of money in an economy with wage and price flexibility, allowing for the wealth effect of price-level changes. Such investigation is the object of the two major works in monetary theory published since the *General Theory*—Patinkin's *Money, Interest and Prices* and Gurley and Shaw's *Money in a Theory of Finance*. These show (the one largely by inference, the other directly) that in an economy in which a variety of assets exists and money is created by purchase of such assets by a banking system, changes in the supply of and demand for money have a long-run and not merely a short-run influence on the real equilibrium of the economy. The present position can be summarized in the statement that Keynes was right to attack Say's Law, but he attacked it for the wrong reason. Properly understood, the significance

of Say's Law is not that it makes the decision to save identical with the decision to invest but that it excludes money altogether from any influence on economic behavior.

2. *The General Theory of Income and Employment.* I have been discussing the *General Theory* as a monetary theory; but its main purpose and contribution is the theory of income and employment. Keynes's great achievement was to cut through the conceptual complexity and literary looseness of contemporary monetary theory to an aggregative general equilibrium model of the economy which, once grasped, was simple, readily manipulable, and above all relevant to contemporary problems. The elements in this model, aside from liquidity preference and the quantity of money are the propensity to consume, the investment demand schedule, and the aggregate supply schedule relating employment to output. Keynes's analysis of the last two of these was thoroughly classical in general outline—extraordinarily so in view of the contemporary eruption of monopolistic and imperfect competition theory. The novel and intriguing element in the theory was the propensity to consume, together with its alter ego, that inexhaustibly versatile mechanical toy, the multiplier.

The propensity to consume made the theory of income determination a simple theory in which income was determined by the amount of fixed capital investment, the multiplier playing a role analogous to that of velocity in the quantity theory. The concept of income as the main determinant of expenditure, which Keynes confined to personal consumption behavior, lent itself readily to extension by others to the spending behavior of government, of corporations, of nations in their external transactions, and of an economy disaggregated into output-producing or income-receiving sectors. The statistical estimation of the consumption function offered itself as an important exercise for the emerging discipline of econometrics; and early analysis of time-series and cross-section data seemed abundantly to confirm the hypothesis that consumption is a stable function of income.

Alas for the consumption function, it dismally failed the test of forecasting postwar unemployment. This failure, together with the paradox disclosed by Kuznets' data on the long-run constancy of the savings ratio, prompted a rapid independent development of the theory of the consumption function, and led to substantive modifications of Keynesian income theory. Both developments have been concerned with the same shortcoming of the theory as Keynes presented it, the neglect of the influence of wealth on consumption, a neglect inherent in Keynes's short-period approach and concealed by his deduction of the shape of the propensity to consume from an unexplored "psychological law." The various hypotheses used to reconcile the short-run variability

with the long-run constancy of the savings ratio, ranging from "secular upward drift" through the influence of highest previous income or value of assets to the more intellectually exciting life-plan and permanent income theories, are all concerned at one or another level of sophistication with the influence of wealth on consumption. Correspondingly, it has become customary to include the value of wealth in some form among the variables on which the Keynesian behavior propensities depend, both to give explicit recognition to the influence of wealth on consumption in the long run and to incorporate the neglected wealth-effect of wage and price-level changes.

Meanwhile, the propensity to consume and the multiplier have dwindled to relative insignificance both in the purer sort of monetary theory and in popular Keynesian economics. For pure theory, the essential Keynesian concept is the functional dependence of aggregate expenditure on itself in its income-generating capacity. The division of expenditure into consumption and investment is a superfluous complication once one drops the restrictive assumption that consumption depends only on income and investment only on the rate of interest, and permits both to change autonomously; similarly, the multiplier is a tiresome way of comparing general equilibrium positions. At the popular level, the essential Keynesian idea is the dependence of income, employment, and (more recently) the rate of inflation on the level of aggregate spending, together with the understanding that economic policy can attack spending at a variety of points; the notion of consumption as a passive respondent to investment is appropriate, if at all, to a laissez faire society, not to one with conscious economic policies. But the propensity to consume survives as an integral part of modern business cycle and growth theory; it has also become a basic component of the theory of planning economic development.

I turn now from the propensity to consume to the equilibrium of income and employment it helps to determine. As Keynes himself indicated in his chapter on "Changes in Money Wages" and as other writers have demonstrated rigorously, underemployment equilibrium in Keynes's system depends on wage rigidity, except in the two possible empirical cases of perfectly interest-elastic liquidity preference and perfectly interest-inelastic consumption and investment demands. Subsequent criticism based on the wealth-effect on demand of a lower price level has circumvented these two exceptions and shown that Keynesian unemployment equilibrium depends on wage rigidity unless the wealth-effect peters out before full employment is reached—an empirical possibility which only a few diehards have been prepared to defend. This demonstration does not controvert Keynes's main contentions about wage reduction as a means of increasing employment in a competitive

economy—that the money wage level influences employment through its monetary effect and not by altering real wages, that in practice wage reduction is difficult to achieve and may influence expectations adversely, and that normally monetary expansion can accomplish the same results more easily and justly. But it does mean that "unemployment equilibrium" has to be reinterpreted as a disequilibrium situation in which dynamic adjustment is proceeding very slowly; this is the interpretation of mathematical economists such as Leontief, Patinkin, and Clower, and is, I believe, a fair modern translation of Keynes's short-period equilibrium technique. Empirical research has confirmed that wage adjustment is slow in depressions and has also shown the "real balance effect" to be small.

A more relevant question is whether large-scale unemployment is the typical situation of an advanced capitalist economy, as the theme and prevailing tone of the *General Theory* imply, and as the stagnationists of the late thirties insisted. It is a particularly relevant question because Keynes, unlike many of his followers, was prepared to concede that traditional quantity theory becomes relevant under full employment conditions. A conclusive argument on this question is impossible, given the changes brought about by massive peacetime armament expenditures, social security and farm support programs, aid for the underdeveloped, and the success of the Keynesian revolution in securing recognition of governmental responsibility for full employment. Nevertheless, I believe that Keynes drastically overgeneralized a particularly bad depression which was made worse by errors of economic policy. Whether this is so or not, mass unemployment of the thirties variety has not been a problem of advanced capitalist countries since the war. Stagnationists do still exist in the modern world; but they are concerned either with the underdeveloped countries or with the failure of capitalism to grow as fast as the Russians. In either case they are certainly not underconsumptionists.

If the consumption function is nowhere near as simple as Keynes made it out to be and underemployment equilibrium is a special case of dynamic disequilibrium and anyway not the chronic problem of modern capitalism, what is left of the general theory of income and employment? The contribution of the *General Theory* to modern economics is certainly not Keynes's specific model of income determination, for not only is his consumption function too simple but his theory of investment is incomplete and has had to be extended to make it usable. Rather the contribution lies in the general nature of Keynes's approach to the problem of income and employment. In the first place, he concentrated attention on the expenditure-income and income-expenditure relationships, which are much easier to understand and

apply than the quantity theory relationships and which provide, in the multiplier analysis, a key to dynamic processes of change. In the second place, he provided a useful macroeconomic general equilibrium model for the analysis of a monetary economy in which capital accumulation is a specialized activity financed by the issue of marketable securities. In pure monetary theory, Keynes's crucial distinction between consumption and investment decisions has been dropped and the model refined into the four-market system comprising goods, labor, money, and "bonds"—two flows and two stocks—but the distinction remains essential to cycle and growth theory. Indirectly, also, Keynes stimulated the development of modern dynamic theory. Finally, what is most important for scientific economics but can easily be used to denigrate Keynes's work, he set out his theory in a model in which the important variables and relationships are specified in a form suitable for statistical measurement and testing. The stimulation given by the *General Theory* to the construction and testing of aggregative models may well prove to be Keynes's chief contribution to economics in the longer perspective of historical judgment, since the application of capital rather than income concepts to monetary theory may well produce better and more reliable results, and the present predominance of the income-expenditure approach prove to be a transitional stage in the analysis of economic behavior.

III. *The Policy Implications of the* General Theory

This brings me to the policy implications of the *General Theory*, which I have hitherto postponed discussing. At this date there is no need to labor the point that the *General Theory* deserves much of the credit for the fact that the maintenance of high and stable employment is now accepted as a governmental responsibility, or that Keynes's theory of effective demand is the origin of the modern theory of economic policy. What calls for comment, rather, is the bias that the majority of Keynesians have drawn from the *General Theory* against allowing money, and consequently monetary policy, an important role in determining the level of activity of the economy. This bias has meant that Keynesian theory has proved a poor guide to the dominant postwar policy problem of inflation and that the Keynesian approach to this problem has tended to degenerate into a confused and often obstructive eclecticism. Now, a bias against money and monetary policy was not characteristic of Keynes's work as a monetary theorist —rather the opposite—and money and the demand for it play an essential role in the *General Theory* itself. It is true that the presentation of the theory plays down the role of money and that despite its title the book contains almost nothing on the theory of inflation; but

Keynes did state clearly that full employment conditions would require a different analysis, and he had after all dealt extensively with such conditions in the *Treatise*. (The *Treatise* is in fact much more relevant to postwar conditions than the *General Theory*—but that would require another lecture.)

It is an interesting question why a theory in which money is important should have turned into the theory that money is unimportant. Part of the explanation lies in certain features of the *General Theory* I have already mentioned that diverted attention from the influence of money and of price expectations on spending. Part of it lies in the hardening of certain of Keynes's conclusions into rigid dogmas in the hands of his disciples—notably the hardening of his legitimate criticisms of the quantity theory into militant opposition to any form of quantity theory reasoning, and the hardening of his opinion that monetary policy might be ineffective in combating a collapse of the marginal efficiency of capital into the conflicting dogmas (*a*) that monetary restriction is dangerous because it might precipitate such a collapse and (*b*) that monetary restriction is useless because it will have a negligible effect on effective demand. Part of it is that for obvious reasons Keynesians have tended to be politically left of center, a position associated with distrust of central bankers—particularly in England, due to the part the Bank of England played in the restoration of the gold standard and the downfall of the first Labour Government. Much of it is simply that the "vulgar Keynesians" seized on the simplest and most striking version of the Keynesian system—autonomous investment and the multiplier—as the essence of it, ignoring the monetary analysis as an irrelevant complication.

Whatever the explanation, the result has been that in analyzing inflation Keynesians have tended to fall back on one or other of two approaches based on components of the *General Theory* rather than on the complete model. One approach is based on the crude, effective demand model of income determination; combined with the expenditure-income-expenditure sequence, this leads into the demand-pull theory of inflation. The other approach is based on Keynes's habit of treating the wage unit as exogenous; combined with the income-expenditure-income sequence, this leads into the cost-push theory of inflation. The one approach leads towards the prescription of fiscal policy to remedy inflation, the other towards the prescription of some form of wage and price control. Neither prescription is very realistic for postwar capitalist economies: fiscal remedies are difficult to graft onto high-level budgets dominated by defense expenditure and structural social welfare programs; wage and price controls are inconsistent with a free enterprise system, and especially with the principle of free col-

lective bargaining. Both approaches, by ignoring or suppressing the monetary side of Keynesian theory, concentrate on the mechanism rather than the causation of inflation; and both virtually assume away the possibility of controlling inflation by monetary means.

Not all Keynesians have been skeptical about monetary policy, especially after experience of it since 1951. Keynesian theory has in fact had a formative influence on modern ideas on monetary policy. The theory of effective demand suggests the question of what precise effects monetary policy has on spending—a question which it is important to ask owing to the tendency of central banks to judge their policies by their effects on interest rates and credit conditions in the markets with which they are immediately concerned. This question has stimulated the search for the effects of monetary policy on particular sectors of the economy and also furnished the rationale for a broader and more selective approach to the techniques of monetary control. On the other hand, the search for specific impacts of monetary policy tends to promote underestimation of its influence; so does the Keynesian concern with interest rates as determinants of effective demand—which also tends to play into the hands of central bankers—since it is only too easy to fall into the habit of identifying an increase or decrease in interest rates with a deflationary or inflationary policy. Similarly, the theory of liquidity preference, in a more indirect way, has played a part in the evolution of the modern theory of central banking, according to which the function of the central bank is the broad one of controlling the liquidity of the economy rather than the narrow one of controlling the quantity of currency and demand deposits. Again, the recognition of the monetary role of financial intermediaries and other credit-granting institutions which this entails can easily lead back to skepticism about the potentialities of monetary policy. But these are matters far removed from the *General Theory*.

IV. *Conclusion*

Let me conclude by summarizing briefly the main points I have made in this lecture. The *General Theory* is an uncommonly untidy book, which bears the strong imprint of the Marshallian tradition from which it sprang. Nevertheless, it has shifted the emphasis of monetary theory to the role of money as an asset with special properties in an uncertain world and forced recognition of the fact that a monetary economy is fundamentally different from a barter economy. It provided a simple and comprehensible aggregative model of the economy, which not only facilitated the analysis of aggregative problems but greatly stimulated the development of econometric work with such models. It explained why the competitive capitalist economy does not automatically main-

tain a satisfactory level of employment and outlined the theory of remedial policy, thereby promoting a revolution in ideas on the responsibilities of government in such a system. On the other hand, the book was weak at a crucial point, in its neglect of the influence of capital on behavior; and its influence has been to distract attention from the role of money in the functioning of the economy. I have not, in this lecture, been able to survey the contributions of Keynes's ideas to the many specialized branches of theory—international economics, public finance, business cycles, economic growth, economic planning, to mention the major ones—where they have proved extremely fruitful. But no one could hope, in a single lecture, to take a census of the progeny of the *General Theory*.

DISCUSSION

DAVID McCORD WRIGHT: Dr. Johnson's paper contains a number of insights. His various points, however, possess a good deal of "countervailing" power, and I believe the best contribution I can make to this discussion is not to dig yet more deeply into terminology and exegesis but rather to summarize as plainly as possible the actual employment problem and what Keynes tried to do with it. In discussing Keynes's ideas, however, it should not be forgotten that there is really quite an illuminating book on the subject, one that seems to give real insight into Keynes's mind. The title of this little-known work is the *General Theory of Employment, Interest and Money* and from it I propose occasionally to quote.

Turning now to the essential problem: In a society with real, net, *ex ante* saving, a flow of technologically "unappropriated" factors of production is constantly being "left" aside by the saving act. Continued full employment requires that this flow of factors be as constantly re-employed by new (real) investment projects. Employment "equilibrium" in such context has none of the character of true equilibrium. Rather it consists of a series of instantaneous balances or offsets. For the fact that there are enough offsets to saving today is no guarantee that a sufficiency will be forthcoming tomorrow. Continual full employment, in fact, usually depends on a continued emergence of a margin of the unforeseen.

This delicate yet vital process (even leaving aside oscillation theory) can be interrupted in several ways: (1) consumption may lag excessively, (2) the money circulation may be interrupted (e.g., the liquidity trap), (3) wage, price, and pressure group rigidities, and/or misguided government policy can slow down or discourage growth and the investment flow, (4) such maladjustments usually cause a collapse of profit prospects on new investment (in Keynesian language the marginal efficiency of capital schedule), and (5) not to be forgotten, prices and productivity for a given country may get out of line internationally.

Keynes, of course, concentrated on (1) and (2), and what he did say about the other three forces—even what he said about them in the *General Theory* —is often forgotten. Yet (1) and (2) are not really fundamental. For a lag in the consumption function (which we now know does not necessarily occur) need not cause trouble if the marginal efficiency of capital schedule has simultaneously shifted upward. And I should judge it almost a fundamental test of a man's economic insight—a *pons asinorum* as it were—if he can explain why investment does not necessarily depend upon consumption, nor consumption upon investment. Space is lacking to quote from the *General Theory* the numerous instances in which this fact is recognized.

Nor is (2), the liquidity trap, fundamental. For what matters here is the level of interest relative to the perspective of profit on new investment projects, and you may search the neo-Keynesian scriptures with lamps for any

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adequate theory of the assumed collapse of the marginal efficiency of capital. Yet this collapse—the perspective of cost-price relationships—is *the* fundamental question.

Concerning money, however, I diverge briefly to point out that Johnson like many others has missed one of the most important factual relations. The transactions motive of L_1 does not depend passively on today's actual income level but is also greatly influenced by expectations concerning tomorrow's income level—especially when the marginal efficiency of capital schedule has shifted. As Keynes said (page 299, the *General Theory*), "Effective demand corresponds to the income the *expectation* of which has set production moving" (italics added). We have here, as Keynes admitted to me, a much more direct relationship between the M.E.C. schedule, liquidity preferences, and the interest rate than usually recognized by Keynesians. (See my "Future of Keynesian Economics" in the June, 1945, issue of the *A.E.R.* In this connection, I should like to say that the demonstration that credit manipulation and monetary forces can affect the long-range rate of interest via the rate of saving would have been no "news" to the classical economist.) As usual, Keynes's end-of-the-war remarks to John H. Williams that he was trying to educate the English to the need for a higher rate of interest have been forgotten.

Returning now to profit prospects, it may shock Dr. Johnson to hear me say that I believe secular stagnation is quite possible—now! But not, I add, for Keynesian reasons. If, however, pressure groups of various sorts, plus international maladjustments, prevent adaptation and slow down both growth and investment, while on the other hand the attempt to save continues, a more or less indefinite institutional underemployment stalemate is quite possible. Incidentally, this is the actual, factual situation Keynes was trying to explain.

But here we find the real weakness of Keynesianism. For in the sort of high relative-cost plus gold outflow maladjustment we are experiencing now and England experienced in the twenties, the real need is for greater productivity, lower costs, social adaptation. But Keynes instead draws attention off into monetary blind alleys, and keeps us from realizing, or trying to remedy, the industrial, even cultural, arteriosclerosis that is the real source of the problem. In thus misdirecting attention, Keynesianism, as usually understood, is now, as it was then, not just a negative inadequacy, but a positive evil.

Yet it would not be fair to end on this purely negative note. There is another Mr. Keynes in the *General Theory*, though admittedly a junior partner. *He* is the man who points out that money wages can be too high. Thus, as he remarked in a later explanation: "I do not want to see money wages forever soaring upward to a level to which real wages cannot follow. It is one of the chief tasks ahead of our statesmanship to find a way to prevent this." This Mr. Keynes also admitted that sometimes general money wage reduction might have a direct favorable effect on production via the marginal efficiency of capital and wrote (page 265, *General Theory*): "when we enter on a period of weakening effective demand, a sudden large reduction of money wages to a level so low that no one believes in its indefinite continuance, would be the

event most favourable to a strengthening of effective demand." This Mr. Keynes also hedged his "paradox of thrift" by his explanation (page 111) that it assumed "no favourable change in the demand schedule for investment" and made many other concessions. This Mr. Keynes will be found emerging in my *Future of Keynesian Economics*. And whatever might be thought of that production, it should not be forgotten that it was approved by John Maynard Keynes himself.

ABBA P. LERNER: I am, of course, in general agreement with Professor Harry Johnson's estimation of the importance of Keynes's book in stimulating progress in aggregative economics, in making it impossible for the effects of spending on earning and vice versa to continue to be ignored, and in sparking the revolutionary assumption of responsibility for economic prosperity by the governments of the free world. I also share Johnson's view of the untidiness and the manifold confusions of the book. But it seems to me—and perhaps this marks me as a fanatical Keynesian—that many of his strictures are unduly severe. Like Professor Klein, I feel that almost all of the "revisions" of Keynes are to be found as implications or as hints in the book; and if Keynes had spelled them out fully, the book would have been three times as long and certainly less than one-third as influential.

Thus, the Cantabrigian innocence of capital theory appears to me primarily as merely a distaste for the jargon—much of the substance being there in more ordinary language. Nor do I see cause for censure in Keynes's supplementing a theory of the proximate determinants of income and employment with a theory of how these determinants (i.e., the consumption, investment, and liquidity functions) are themselves subject to cyclical and secular influences. After listening to Johnson, I remain of the opinion that the liquidity preference theory of interest makes more sense than the loanable funds theory, never having been able to get clear enough or stationary enough a picture of what is meant by loanable funds. I can see that when the supply of money is equal to the demand, people will not be trying to change their money holdings, and this may be what is meant by equilibrium in the loanable funds market. But I cannot see how a shift in the supply or demand for loanable funds could change the equilibrium rate of interest other than through a change in the supply or demand for money. An understanding of the rates at which people adjust their money holdings in moving toward the equilibrium position while the higgling of the market is proceeding would indeed add something valuable, but I am not aware that anyone has figured out how the loanable fund theory or any other device could handle such complicated dynamic analysis.

I would have similar reservation on a large number of other points which, I am sure, will be most helpful for discussions in the Money Workshop at Michigan State University, but I would rather concentrate here on an area where Johnson misses a chance to point out a really serious incompleteness in the Keynesian scheme, pausing only to register my surprise at Johnson's interpretation of the alleged Keynesian distaste for monetary policy. As an old-

time Keynesian, one of the hardest crosses to bear was the popular reduction of the *General Theory* to the Keynesian Special Case of a depression so severe that monetary policy is ineffective and fiscal measures are called for to restore demand. Keynes's application of the general theory to wartime and postwar inflationary situations thus came to be interpreted as a repentance for his sins and a return to classical orthodoxy. For Johnson to blame Keynes or the Keynesians for this caricature is indeed a bit thick.

It would indeed be improper to blame Keynes for the incompleteness I want to discuss, but the failure to see it twenty-five years later seems to me to betray a fundamental misunderstanding of the place of price rigidity in the whole Keynesian system.

The price rigidity blamed by pre-Keynesians for unemployment was the refusal of wages to fall the very little that would be necessary to induce the absorption of the unemployed by firms faced by constant product prices. The futility of this order of price flexibility was exposed by Keynes's pointing out that prices would not remain the same, while the dynamic expectations aroused would make things worse rather than better. This was the wage rigidity on which the "unemployment equilibrium" did *not* depend.

A second order of price flexibility would involve a much greater reduction of wages *and of product prices* relatively to a supposedly stable level of money income, but Keynes pointed out that the reduction of wages and prices constitute a parallel reduction in income.

A third order of price flexibility would call for a still greater reduction of wages, prices, and money incomes relatively to the money stock—one which would increase liquidity in the economy sufficiently to increase spending sufficiently to restore full employment. This order of price flexibility would in general really cure the unemployment; and if price rigidity means the absence of this third order of price flexibility, then the unemployment equilibrium *is* dependent on price rigidity. But even this third order of price flexibility would fail to restore full employment if a very high elasticity of liquidity (the liquidity trap) prevented the rate of interest from falling or if a low elasticity of investment (collapse of confidence) made the fall in interest ineffective. In that case the alternative indicated by the Keynesian analysis (an increase in the stock of money to yield the same increase in liquidity without putting the economy through the wringer) would also fail. This is a variant of the Keynesian Special Case where fiscal policy has to bail out monetary policy. The fiscal measures might involve an increase in the money stock, but that would be "mere window dressing."

Some of you may remember a meeting of this Association at which the late Professor Hardy turned this around and considered the opposite case—the Classical Special Case—where the elasticity of investment is high, the elasticity of liquidity is low, only monetary policy is effective and fiscal policy is mere window dressing. On that occasion I was moved to tell the story of the debate whether the sun or the moon was more important and its conclusion that the moon was much more important because it shone in the dark when it was needed. The purpose of the story was to suggest that both special cases

were encompassed in the *General Theory*. It now seems to me that the sun and the moon have both been eclipsed and we are in a situation where monetary and fiscal policy are both inadequate.

But I am anticipating. I must first consider a fourth order of price flexibility, much discussed recently and stressed heavily by Johnson, which would cure depressions even where the third degree failed. It would do this by increasing the real wealth of the community. The owners of any existing dollars or government bonds would become so rich that they would consume enough to restore full employment.

It was precisely in order to point out the impossibility of the third and fourth orders of price flexibility that Keynes wrote the chapter (17) dismissed by Johnson as pretentious philosophizing. Any monetary system that permitted third- or fourth-order flexibility would go the way of the moneys that were driven out by better moneys when they lost their stability in hyperinflations. Keynes was an impatient man and had no use for academic philosophizing about impossible degrees of flexibility, and I am convinced that it was this impatience and not a failure to consider the theoretical possibility that is responsible for his not mentioning the "wealth effect." Johnson's suggestion that neglect of the wealth effect was responsible for the falsified expectation of postwar depression is not convincing. Certainly the hunger for automobiles played as large a part as the accumulation of cash and war bonds, and the growth of claims was not brought about by a fourth-order price flexibility. Indeed, the hypersophisticated argument that the wealth effect saves the pre-Keynesian classical belief in automatic full employment (to which belief Johnson's talk could give much aid and comfort) sounds like a Ptolemaian using Einstein to refute Copernicus, arguing that the earth could not be moving round the sun because we would be crushed by the increased mass from the high velocity.

The trick missed by Johnson, however, was not in the degree of flexibility but in the nature of the inflexibility. If, as Keynes said, prices are "inflexible downward," this is because they are determined not by "the market" or the laws of supply and demand, but by the officers of corporations, trade-unions, and the like, who decide that wages or prices should not fall when the market would have said that they should. What Keynes did not see was that, if people have the power to tell prices to stop when they should go, they also have the power to tell them to go when they should stop. They may raise prices when there is no excess demand and even when there is a deficiency of demand.

Where there exists, furthermore, a social climate that encourages increases in wages and prices for political, organizational, sentimental, traditional, and many other reasons, prices will rise; and governments that have assumed responsibility for economic prosperity will provide any extra cash balances needed to maintain satisfactory employment levels at higher price levels.

The treatment of this kind of inflation can follow three paths. The first is to make conditions more competitive. This is desirable for many reasons, but efforts in this direction are not likely to outstrip the rapidly developing art of large-scale organization that makes oligopoly more natural and price administration possible.

The second path is to achieve price stability by regulating effective demand, using fiscal or monetary policies or both. This does not work whichever of the two is stressed, because the level of effective demand that yields a satisfactory level of employment is not the same as that which provides a stable price level—Keynes's assumption (that it is) notwithstanding. The consequent frustrations of governments, who find that contraction of effective demand causes depression before it stops inflation and that expansion brings inflation before it cures depression, are responsible for the current revival of budget-balancing fundamentalism. And that is starting a fashion among economists for sophistical justifications of these superstitions by redefinition of terms reminiscent of attempts to adjust Genesis to geology by redefining the "day."

The third path is to force the price administrators to follow criteria of social desirability which would make full employment compatible with a stable price level. Such regulation is almost the exact opposite of "price control" in that instead of attempting to frustrate the operation of the free market's "invisible hand" it interferes only with the interferences with the market by the price administrators. But as long as economists of even Johnson's virtuosity dismiss such an extension of the public utility pricing principle on the ground that it is "incompatible with free enterprise," the chances of its adoption are slight. The unhappy prognosis is that we will continue to prepare the world for Communist tyranny and/or nuclear destruction by suffering the increasing degree of depression that our increasing productivity will permit; and that we will justify this as the only legitimate medicine for moderating inflation, when we could be utilizing our magnificent economic potential for economic growth and the building of a prosperous and secure free world.

LAWRENCE R. KLEIN: We are indebted to Harry Johnson for an admirable statement of the Keynesian methodology in the *General Theory* and its influence on later developments in the technique of economic analysis. He has put these matters very nicely, and I can agree with these aspects of his address, but his statement of the Keynesian theory and its substantive significance for economic analysis is unacceptable. Maybe money is as important as Harry Johnson says it is, but he has a lot of proving to do before I shall believe it. It is all very well to talk about the marriage of the real and the money economy, but has he actually shown how an effective union can be forged? The real economy is still the dominant force in our life. It is probably true that the money and capital parts of the Keynesian theory are in the least satisfactory state, requiring future work, but this is hardly an adequate reason for putting them in the limelight on the occasion of the silver anniversary.

Johnson writes, "Alas for the consumption function; it dismally failed the test of forecasting postwar employment." To this, I reply, "Alas for monetary theory and monetary policy; they dismally failed to help understand or halt postwar inflation."

Let me now hack away at specific points of his paper. He regards the debates on stock versus flow theories of interest and liquidity preference versus loanable funds theories as tiresome and trivial. It is true that stock and flow

theories are related and have some equivalences, but it is no trivial or automatic matter to bring out the proper relations between the two. It is an open question whether the interest rate fluctuates in response to an excess (deficit) of loanable funds or an excess (deficit) of money stock. It is intuitively obvious to Professor Johnson that excess supply or demand for loanable funds is the key variable, but he will have to demonstrate this more convincingly. He accuses Keynes of being misleading in claiming "that holding securities is the only relevant alternative to holding money, and that the classical alternatives of spending money on consumption or investment play no part in determining the demand for money and the rates of interest." Has he not confused stocks and flows here? Surely, the appropriate alternatives are the holding of goods, money, and securities—the last in many types. The consumption-investment alternatives are flows. He would do better to state his theory consistently in terms of stocks.

Professor Johnson condemns aggregation in the Keynesian analysis of money and interest. But Keynes was dealing with an aggregative system. If we accept this mode of analysis, we must be prepared to have errors of aggregation. Why is aggregation any worse in the money than in other markets? Harry Johnson is willing to deal with aggregate employment, aggregate output, the general wage rate, and the general price level. There are serious aggregation problems in the study of factor share ratios of income distribution. Unless he is prepared to develop a detailed micro system with all the fine interrelations, he must reckon with aggregation error.

Have Keynesian writers neglected the influence of price-level expectations on asset choices? The first studies of empirical liquidity preference functions by A. J. Brown, right after the publication of the *General Theory*, introduced price movements as a variable explaining money holdings. Many econometric studies since then, all in the Keynesian tradition, have done similarly. Professor Johnson is way off the track here.

When Keynes first proposed the idea of the consumption function as a simple and basic psychological law he pointed out many qualifications for things that would affect consumer behavior but were held constant in his analysis. He was well aware of the influence of income distribution, changing prices, lags, taxes, etc., in effecting consumption. Practically all the work on the consumption function has been a systematic follow-up of these leads suggested by Keynes. The most satisfactory of the amended consumption functions now take account of factor share distributions, lags, price level, and taxes. What could be more natural in the development of a book's ideas than to follow up on the leads suggested by the author? Even the permanent income theories that Harry Johnson finds "more intellectually exciting" have not amounted to more than distributed lag functions. These in no way seriously modify the content of Keynesian theory except by dynamizing it in a natural way suggested by Keynes.

The only modification of the consumption function that has essential significance is the inclusion of the wealth or real balance effect. It is easy to make too much of this, as Johnson does. Maybe just after the war when there was pent-up demand and very low consumer stocks, coupled with large hold-

ings of liquid assets, there was evidence of a wealth effect. It is not nearly as important now. It has not been found to be important in other countries. We disposed of the interest effect on savings or consumption early in the Keynesian debate by means of a logical argument showing that the effect could go either way (positive or negative) depending on rational choice of savings plans by individuals. I think that the same type of argument might be applied to the wealth effect. The rich are schooled in the virtues of thrift and accumulation. With them it may well be that "the more they have the more they want." In the lower income groups the effect is likely to be just the opposite.

Is not Professor Johnson guilty of generalizing the situation of the fifties, as regards the wealth effect and the maintenance of full employment, in the same way that Keynes and the Keynesians are guilty of generalizing the conditions of the thirties? We economists often try to stretch a decade into a whole era. Johnson's ideas are not more likely than Keynes's original thoughts to stand the test of time. My guess is that Harry Johnson is on much weaker footing when he says that the income-expenditure approach may prove to be a transitional stage in the analysis of economic behavior.

Harry Johnson finds the multiplier to be a "mechanical toy" and "tiresome." He also writes: "Meanwhile, the propensity to consume and the multiplier have dwindled to relative insignificance in both the purer sort of monetary theory and popular Keynesian economics." What can he possibly mean by this remark? Obviously, the appearance of such concepts in the established textbooks reaching thousands of students annually have made them extremely important in popular discussion. They were never so popular in Keynes's lifetime.

Professor Johnson finds the division of expenditure into consumption and investment to be a superfluous complication. He gets himself into gross and needless aggregation complications. This distinction continues to be of extreme importance in the treatment of inflation, business cycle prediction, and the analysis of growth. Does consumption spending contribute to capital accumulation, further productivity and growth in the same way as investment?

I like Johnson's characterization of the Keynesian system as a disequilibrium system. It is better viewed as a dynamic model. Then the question of underemployment equilibrium is not necessarily a matter of wage or price rigidity. The essence of the matter is that the dynamic system has no stable equilibrium—no equilibrium of full employment.

At this stage, it is easy to look back and deprecate or belittle the Keynesian contribution. I am tremendously impressed by the extent to which I continuously draw upon the *General Theory* in my daily work. It is truly a great book.

MONETARY THEORY: NEW AND OLD LOOKS

MONEY, CAPITAL, AND OTHER STORES OF VALUE

By JAMES TOBIN
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I. *Monetary Economics and Rational Behavior*

The intellectual gulf between economists' theory of the values of goods and services and their theories of the value of money is well known and periodically deplored. Twenty-five years after Hicks's eloquent call for a marginal revolution in monetary theory [4] our students still detect that their mastery of the presumed fundamental, theoretical apparatus of economics is put to very little test in their studies of monetary economics and aggregative models. As Hicks complained, anything seems to go in a subject where propositions do not have to be grounded in someone's optimizing behavior and where shrewd but casual empiricisms and analogies to mechanics or thermodynamics take the place of inferences from utility and profit maximization.

From the other side of the chasm, the student of monetary phenomena can complain that pure economic theory has never delivered the tools to build a structure of Hicks's brilliant design. The utility maximizing individual and the profit maximizing firm know everything relevant about the present and future and about the consequences of their decisions. They buy and sell, borrow and lend, save and consume, work and play, live and let live, in a frictionless world; information, transactions, and decisions are costless. Money holdings have no place in that world, unless possession of green pieces of paper and yellow pieces of metal satisfies some ultimate miserly or numismatic taste. Wealth, of course, has the reflected utility of the future consumption it commands. But this utility cannot be imputed to money unless there are no higher yielding assets available. As Samuelson has pointed out [4, pages 122-24], in a world of omniscient households and firms, dealing in strictly perfect markets, all vehicles of saving in use must bear the same rate of return. If "money" bears that yield, wealth-holders will be indifferent between money and other stores of value—the demand for money will be indeterminate. If money fails to yield the going rate, no one will hold it. Even though money is required as a medium of exchange, transactors will suffer no cost or inconvenience by holding more lucrative assets at all times except the negligible microseconds before and after transactions.

The general sources of the "utility" of money have, of course, long been clear to monetary theorists. Lavington [9] and Pigou [13], for example, imputed to money a rate of return varying inversely with the size of money holdings relative to the transactions needs and total wealth of the holder. This return stands for the convenience and economy of having wealth readily available as means of payment, as well as the safety of money compared with other stores of value. The only alternative asset that these elders of the Cambridge School explicitly envisaged was capital investment. "This proportion [k] depends upon the convenience obtained and the risk avoided through the possession of [money], by the loss of real income involved through the diversion to this use of resources that might have been devoted to the production of future commodities. . . . k will be larger the less attractive is the production use and the more attractive is the rival money use of resources. The chief factor upon which the attractiveness of the production use depends is the expected fruitfulness of industrial activity" [13, pages 166, 168]. In short, an individual adjusts his money holding so that its marginal imputed return is equal to the rate available to him in capital investment. Paradoxically the Cambridge tradition did not build on these ideas of liquidity preference. Instead of being systematically related to the profitability of investment and to other variables affecting the rational calculations of wealth owners, the demand for money became a constant proportion of income. Marshall [11, page 47] had explicitly mentioned wealth as well as income, but somehow wealth was dropped from the tradition. (k is not the only instance in English economics where a variable coefficient left unprotected by functional notation has quickly evolved into a constant in everyday use.) Hicks's prescription for monetary theory in 1935 was in much the same spirit as the approach of Lavington and Pigou. His strictures were nonetheless timely; the spirit of the original Cambridge theory had become obscured by the mechanical constant-velocity tradition.

Recent developments in economic theory have greatly improved the prospects of carrying out Hicks's "simplifying" suggestions and deriving rigorously the imputed return or marginal utility of money holdings in relation to their size. In the past decade theory has begun a systematic penetration of the murky jungle of frictions, market imperfections, and uncertainties. The theory of optimal inventory holdings, for example, shows how transactions and delivery costs must be balanced against interest and carrying costs. Applied to inventories of cash, the theory gives precision to the relation of cash holdings to the volume of nonfinancial transactions, the costs of asset exchanges, and the yields available on alternative assets [1] and [17]. A parallel de-

velopment has been the theory of choices involving risk. Applied to the general strategy of portfolio selection, the theory of risk aversion explains how money may find a place in a rationally diversified portfolio [10] and [18].

The new tools are constructing a bridge between general economic theory and monetary economics. More than that, they give promise at last of a general equilibrium theory of the capital account. Such a theory would explain both the balance-sheet choices of economic units as constrained by their net worths and the determination of yields in markets where asset supplies and demands are balanced. What characteristics of assets and of investors determine the substitutabilities or complementarities among a set of assets? Among the relevant properties with which the theory must deal are: costs of asset exchanges; predictability of real and money asset values at various future dates; correlations—positive, negative, or zero—among asset prospects; liquidity—the time it takes to realize full value of an asset; reversibility—possibility and cost of simultaneously buying and selling an asset; the timing and predictability of investors' expected needs for wealth.

In a world of financial assets and well-developed capital markets, Keynes [7, pages 166 and 168, pages 140-41] was right in perceiving the tactical advantage to the theorist of treating separately decisions determining total wealth and its rate of growth and decisions regarding the composition of wealth. A theory of the income account concerns what goods and services are produced and consumed, and how fast nonhuman wealth is accumulated. The decision variables are flows. A theory of the capital account concerns the proportions in which various assets and debts appear in portfolios and balance sheets. The decision variables are stocks. Income and capital accounts are linked by accounting identities—e.g., increase in net worth equals saving plus capital appreciation—and by technological and financial stock-flow relations. Utilities and preference orderings attach to flows of goods and services; the values of stocks are entirely derivative from their ability to contribute to these flows. Some stock-flow relationships are so tight that this distinction is pedantic: the only way an art collector can obtain the flow of satisfactions of owning a particular *chef d'oeuvre* is to own it. But there is a vast menu of assets whose yields are generalized purchasing power, nothing less or more—investors do not have intrinsic preferences among engravings of security certificates.

II. *The Capital Account in Aggregative Models*

Strictures on the Need for Explicit Assumptions. Aggregative models of the income account reduce the dimensions of general equilibrium theory, purchasing definiteness in results at the risk of errors of aggre-

gation. Commodities, prices, and factors of production are limited to one or two. For similar reasons, it is fruitful to limit the number of assets in aggregative theory of the capital account.

The first requisite of a theory of wealth composition is that decisions about assets and debts must, in the aggregate as for the individual, add up to the net worth of the moment, neither more nor less. Monetary theory needs to specify explicitly what forms the nonmonetary parts of wealth can take. Many confusions and disagreements can be traced to ambiguities and differences in assumptions about the nature of wealth. A theory should state the menu of assets assumed available, specifying which are components of net private wealth (capital stock plus government debt) and which are intermediate assets (private debts). Moreover, the independent interest rates in an aggregative system should be enumerated. An independent rate is one that is not tied to another yield by an invariant relationship determined outside the system; e.g., by a constant risk differential.

The means of payment of a country—at least in part governmental in origin—are generally demand "debts" of the central government. But there are also means of payment of private manufacture; indeed it is possible to imagine a pure credit economy without government debts of any variety, where all means of payment are private debts backed by private debts. Likewise it is possible to imagine a wholly nonmonetary public debt.

Monetary discussions suffer from confounding the effects of changing the supply of means of payment with the effects of changing the net value of private claims on the central government. The second kind of change takes time and requires private saving, absorbed in fiscal deficit, or dissaving equal to fiscal surplus. The first type of change can be accomplished instantaneously by exchanges of assets. When an author proposes to discuss the effects of changing the supply of money, is he imagining aggregate net worth to change simultaneously by the same amount? Effects that are due to increases of private wealth in the form of government debt should not be attributed to money per se. Sometimes we are asked to imagine that everyone wakes up to find his cash stock has doubled overnight and to trace subsequent adjustments. This mental experiment is harmless and instructive, provided its results are not considered indicative of changes in money supply engineered by normal central bank procedures. The overnight miracle increases equally money stocks and net worth; the gremlins who bring the money are not reported to take away bonds or IOU's. The repercussions are a mixture of effects: partly those of an unanticipated increase in net worth in the form of assets fixed in money value (as if the gremlins had brought bonds instead); partly those of an in-

crease in the supply of means of payment relative to transactions needs and to other assets. The theory of real balance effect [12] is at the same time much more and much less than the theory of money.

Established procedure in aggregative model building is to specify the quantity of money, M, as an exogenous variable determined by the "monetary authorities." The practice is questionable when part of the money supply is manufactured by private enterprise. Banks are not arms of government. The true exogenous variables are the instruments of monetary control: the quantity of demand debt available to serve as primary bank reserves, the supplies of other kinds of government debt, required reserve ratios, the discount rate. Once these instrument variables are set, the interaction of bank and public preferences determines the quantity of money. No doubt a skillful central bank can generally manipulate its controls to keep M on target, but part of the job of monetary theory is to explain how. A theory which takes as data the instruments of control rather than M, will not break down if and when there are changes in the targets or the marksmanship of the authorities.

Two Models, One Keynesian and One Not. The assets of a formal model of Keynes's *General Theory* [7] appear to be four or possibly five in number: (1) government demand debt, serving either as means of payment or as bank reserves, (2) bank deposits, (3) long-term government bonds, (4) physical capital, i.e., stocks of *the* good produced on the income-account side of the model, and possibly (5) private debts, serving along with bonds (3) and demand debt (1) as assets held by the banking system against its monetary liabilities (2). Net private wealth is the sum of (1), (3), and (4).

Though there are four or five assets in this model, there are only two yields: the rate of return on money, whether demand debt or bank deposits, institutionally set at zero, and *the* rate of interest, common to the other two or three assets. For the nonmonetary assets of his system, Keynes simply followed the classical theory of portfolio selection in perfect markets mentioned above; that is, he assumed that capital, bonds, and private debts are perfect substitutes in investors' portfolios. The marginal efficiency of capital must equal *the* rate of interest.

Keynes did not, of course, envisage literal equality of yields on consols, private debts, and equity capital. Indeed, he provides many perceptive observations on the sources and cyclical variations of the expectations and risk premiums that differentiate market yields. But in given circumstances these differentials are constants independent of the relative supplies of the assets and therefore inessential. Once one of the rates is set, the others must differ from it by appropriate allowances for risk and for expectations of price changes.

Thus Keynes had only one yield differential to explain within his theoretical model: the difference between the zero yield of money and *the* interest rate. This differential he explained in his theory of liquidity preference, which made the premium of bond yields above money depend on the stock of money relative to the volume of transactions and, presumably, aggregate wealth. Keynes departed from the classical model of portfolio choice and asset yields to explain money holdings, applying and developing an innovation borrowed from his own *Treatise* [8, pages 140-44, 248-57], a rate differential that depends systematically on relative asset supplies.

Post-Keynesian aggregative theorists, whether disciples or opponents or just neutral fanciers of models, have stuck pretty close to the Keynesian picture of the capital account. For example, Patinkin [12] explicitly includes all the assets listed above, and no more, in his most comprehensive model. Like Keynes, he has only one interest rate to determine. His difference from Keynes is his real balance effect.

As Hicks [5], Kaldor [6], and others have pointed out, there are apparently no short-term obligations of fixed money value in the Keynesian scheme. Recognition of these near-moneys would add one asset category and a second interest rate to the Keynesian model of the capital account. Transactions costs become the major determinant of the money-short rate differential, and considerations of speculation and risk for investors of different types affect the size and sign of the short-long differential.

An entirely different monetary tradition begins with a two-asset world of money and capital and ignores to begin with all closer money substitutes of whatever maturity. Significantly, the authors of the Cambridge tradition, as mentioned above, regarded direct capital investment as *the* alternative to money holdings. Why did they fail to carry into their monetary theory the clear inference that the demand for money depends not only on the volume of transactions but also on the yield of capital? Perhaps the best guess is that for these economists the yield of capital was in the short run a constant, explained by productivity and thrift. Money balances were adjusting to a rate already determined, not to a rate their adjustment might help to determine.

On its own logic, therefore, the constant-velocity approximation is of little applicability in models where the rate of return on capital is variable. It is not applicable to cyclical fluctuations, where variations of employment affect the productivity of the given capital stock. It is not applicable to secular growth, if capital deepening or technological change alters the yield of capital.

Neither is the constant-velocity assumption applicable where money substitutes other than capital are available and have endogenously

variable yields, for then the demand for money would depend on those yields. Paradoxically, the model of greatest popularity in everyday analysis of monetary policy really has no room for monetary policy per se. In the two-asset, money-capital economy there are no assets which the central bank and the banking system can buy or sell to change the quantity of money.

What is the mechanism by which a change in the quantity of money brings about the proportional change in money income that constant-velocity theory predicts? Sometimes the mechanism as described seems to assume a direct relationship between money holdings and spending on income account: When people have more money than they need, they spend it. It is as simple as that. Patinkin [12, Chapter 8] rightly objects that spending on income account should be related to excess wealth, not excess money. If the mechanism is a real balance effect, then it works only when new money is also new private wealth, accumulated by the public as a result of government spending financed at the printing press or the mint.

A mechanism more in the spirit of the arguments of Lavington, Pigou, and Hicks is that owners of wealth with excess money holdings seek to restore the balance of their capital accounts. Trying to shift from money to capital, they bid up the prices of the existing capital stock; and since new capital goods and old must bear comparable prices, prices also rise in commodity markets. The process ends when, and only when, money incomes have risen enough to absorb the new money into transactions balances. The real rate of return on the capital stock remains unchanged.

This mechanism can apply to increases in M due to expansion of bank lending—with private debts added to the menu of assets—as well as to increases associated with net saving. One aspect of the mechanism is then the process of which Wicksell [19] gave the classical description. Banks expand the money supply by offering to lend at a rate— the market rate—lower than the yield of capital—the natural rate. Excess demand for capital by new borrowers bids up capital values, with the repercussions already described. Whether this process has an end or not depends on whether the banks' incentive to expand is extinguished by proportionate increases of money supply, money income, and prices. For a pure credit economy, where all means of payment are based on monetization of private debts, this model produces no equilibrium. The end to the Wicksellian process depends on banks' needs for reserves, whether enforced by legislation or by their own transactions and precautionary motives.

I have presented a modern version of a two-asset, money-capital economy in [16]. Money and government debt are one and the same,

and there are no private debts. The proportions in which owners of wealth desire to split their holdings between money and capital depend upon the volume of transactions and on the rate of return on capital. The yield of capital is not a constant, as it seems to be in the Cambridge model, but depends on the capital intensity of current production. The differential between the yield of capital and that of money depends on the relative supplies of the two basic assets; the liquidity preference mechanism is applied to a money-capital margin rather than a money-securities margin. The price level adjusts the relative supplies to the portfolios investors desire, given the ruling marginal productivity of capital. This portfolio adjustment is like the mechanism of response to increase in the quantity of money described above for the constant-velocity model; but here it does not necessarily maintain the same velocity or the same yield of capital. A real balance effect on consumption can be added if desired.

A trivial extension of the money-capital model is to include other kinds of government securities, on the assumption that given certain constant rate differentials they are perfect portfolio substitutes for money proper. Then "money" in the model stands for the entire government debt, whether it takes the form of media of exchange or money substitutes. The differential between the return on capital and the yield of any government debt instrument is determined by the relative supplies of total government debt and capital.

By a similar extension private debts could be added to the menu of assets, again with the proviso that they are perfect substitutes for government debt instruments but not for capital equity. This addition does not change the requirement of portfolio balance, that the net private position in assets of fixed money value stands in the appropriate relationship to the value of the capital stock.

Thus extended, the money-capital model winds up with the same asset menu as the Keynes-Patinkin model. Each has only one interest differential to be explained within the model. But there is a vast difference. The Keynes-Patinkin model assumes that all debt instruments are perfect substitutes for capital. The interest rate to be explained is the rate common, with the appropriate constant corrections, to all assets other than money itself. What explains this rate is the supply of money relative to transactions requirements and to total wealth. Monetary policy, altering the demand debt component of government debt, can affect the terms on which the community will hold the capital stock. Expansion of the real value of unmonetized debt cannot do so, although in Patinkin's version it can influence the level of activity via the real balance effect on current consumption. The money-capital model, in contrast, casts debt instruments on the side of money and focuses at-

tention on the relationship between the total real value of government debt, monetized or unmonetized, and the rate of return the community requires of the capital stock. It contains no role for monetary policy; only the aggregate net position of the public as borrowers and lenders is relevant, not its composition.

The two models give different answers to important questions. Does retirement of government long-term debt through taxation have expansionary or deflationary consequences? The question refers not to the temporary multiplier-like effects of the surplus that reduces the debt—these are of course deflationary—but to the enduring effects, through the capital account, of having a smaller debt. The instinctive answer of economists schooled in the Keynesian tradition is "expansionary." The supply of bonds is smaller relative to the supply of money; *the* rate of interest goes down, and investment is stimulated until the marginal efficiency comes down correspondingly. The answer of the money-capital model is, as indicated above, "deflationary." The assumed substitutability of bonds and money will keep the bond rate up. The decline in the government debt component of net private wealth means that investors will require a higher rate of return, or marginal efficiency, in order to hold the existing capital stock.

Granted that both models are oversimplified, which is the better guide to instinct? Are long-term government debt instruments a better substitute for capital than they are for short-term debt and money? Reflection on the characteristic properties of these assets—in particular how they stand vis-à-vis risks of price-level changes—surely suggests that if government securities must be assimilated to capital or money, one or the other, the better bet is money.

Towards a Synthesis. A synthesis of the two approaches must, of course, avoid the arbitrary choices of both, abandoning the convenience of assuming that all assets but one are perfect substitutes. The price of this advance in realism and relevance is the necessity to explain not just one market-determined rate of return but a whole structure. The structure of rates may be pictured as strung between two poles, anchored at one end by the zero own-rate conventionally borne by currency (and by the central bank discount rate) and at the other end by the marginal productivity of the capital stock. Among assets that are not perfect substitutes, the structure of rates will depend upon relative supplies. In general, an increase in the supply of an asset—e.g., long-term government bonds—will cause its rate to rise relative to other rates, but less in relation to assets for which it is directly or indirectly a close substitute—in the example, short-term securities and money—than in relation to other assets—in the example, capital.

In such a synthesis, monetary policy falls in proper perspective. The

quantity of money can affect the terms on which the community will hold capital, but it is not the only asset supply that can do so. The net monetary position of the public is important, but so is its composition.

One lesson of the simple money-capital model should be retained. The strategic variable—the ultimate gauge of expansion or deflation, of monetary tightness or ease—is the rate of return that the community of wealth-owners require in order to absorb the existing capital stock (valued at current prices), no more, no less, into their portfolios and balance sheets. This rate may be termed the supply price of capital. If it is lower than the marginal productivity of capital, there will be excess demand for capital, stimulating increases in prices of capital goods and additions to the stock. If the supply price of capital is higher than its marginal productivity, demand for capital will be insufficient to absorb the existing stock; its valuation will tend to fall, discouraging production of new capital goods. The effects of deviation of supply price of capital from the marginal productivity of the existing stock are similar to those of discrepancies between Wicksell's market and natural rates.

In assessing policy actions and other autonomous changes, there is really no short-cut substitute for the supply price of capital. As the example of long-term debt retirement illustrates, *the* Keynesian interest rate, the long-term bond rate, can be a misleading indicator. Events that cause it to fall may cause the supply price of capital actually to rise. Another example of error due to concentration on the long-term bond rate is the following Keynesian argument: Expectation of a rise in *the* interest rate leads to liquidity preference and keeps the current interest rate high; a high interest rate discourages investment. However, what the marginal efficiency of capital must compete with is not the market quotation of the long-term rate, but that quotation less the expected capital losses. If the fact that the rate so corrected is close to zero causes substitution of money for bonds, should it not also cause substitution of capital for bonds?

If the long-term bond rate is an inadequate substitute for the supply price of capital, the same is true of another popular indicator: the quantity of money. The modern quantity-of-money theorist [2] (to be distinguished from the ancient quantity-theorist-of-money, who actually was a believer in the constancy of velocity), holds that virtually everything of strategic importance in the capital account can be studied by focusing on the supply and demand for money. This view, though seemingly endorsed by Shaw [15], has been persuasively opposed by Gurley and Shaw [3]. As they point out, it is not hard to describe events and policies that raise the supply price of capital while leaving

the quantity of money unchanged or even increasing it. Why concentrate on variables other than those of direct central interest?

How far to go in disaggregation is, as always, a matter of taste and purpose; it depends also on the possibilities of empirical application and testing. A minimal program for a theory of the capital account relevant to American institutions would involve: (1) four constituents of net private wealth: government demand debt, government short debt, government long debt, and capital stock; (2) two intermediate assets: bank deposits and private debts; (3) two institutionally or administratively fixed interest rates: zero on bank deposits and demand debt, and the central bank discount rate; (4) four market-determined yields: the short-term interest rate, the long-term interest rate, the rate on private debts, and the supply price of equity capital.

In this model, the quantity of demand debt is divided between currency held outside banks and the net (unborrowed) reserves of banks. Required reserves depend on the volume of deposits. If required reserves exceed net reserves, banks must borrow from the central bank at the discount rate. The disposable funds of banks are their deposits less their required reserves. These are divided among net free reserves (net reserves less required reserves), short governments, long governments, and private debts in proportions that depend on the discount rate, the short rate, the long rate, and the private loan rate. The nonbank public apportions net private wealth among currency, bank deposits, the two kinds of interest-bearing government debt, private debt to banks (a negative item), and capital equity. All the yields except the discount rate are relevant to the public's portfolio choices. When the wealth constraints are allowed for, there are four independent equations in this system; e.g., a balance equation for each constituent of net private wealth. These equations can be used to find the four endogenous yields. The solution for the yield of capital is its supply price. There is equilibrium of the whole system, which would include also equations for the income account, only if the solution for the supply price of capital coincides with the marginal productivity of the existing stock.

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OUR KNOWLEDGE OF MONETARY POLICY

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My original intention was to offer here a critical survey of recent contributions to our knowledge of monetary policy. Before I had got very far with the writing of a first draft, however, it dawned on me (as indeed it should have before I ever started to write) that a survey sufficiently detailed to be worth while would require many more pages than were available. Reluctantly, then, I have had to content myself with a lesser objective: to discuss what seems to me to be some of the more important gaps in our knowledge of monetary policy. But since judgments about gaps in our knowledge are of necessity based on judgments about what we need to know, I shall have to begin by giving my view of that lengthy and extraordinarily complex causal chain which we call monetary policy.

A View of Monetary Policy

In formulating its economic policy, the Federal Reserve, like all other organizations, must decide what goals it is going to shoot for and what its strategy will be. From among all feasible sets of values which its target (goal) variables can take on, it must select the desired set. And having done this, it must decide which set of values of its instrument variables—those wholly under its control—will yield the predetermined targets [16, Chapter 1]. More concretely, the Federal Reserve must decide whether to aim for a higher, lower, or constant price level, etc.; and it must decide what discount rates and reserve requirements will be and how many Treasury securities it will hold.

Sometimes, of course, other matters have to be decided as well; in the past, the Federal Reserve has had to set down-payment and maturity terms for consumer loans. Mostly, though, the System operates with three instrument variables—a vector of discount rates, a vector of reserve requirements, and a vector the components of which are its holdings of the several maturities of Treasury obligations—and so follows what can be called the classical approach. Thus, to describe the classical portion of current Federal Reserve strategy, it is enough to indicate the current values of the three classical instrument variables. Average values of these variables can similarly be used to describe the strategy of the period over which the averages are taken. It is also possible, however, as a moment's reflection will show, to describe Federal Reserve strategy of a given (past) period by specifying the aver-

age maximum amount of earning assets which member banks as a group could have held. To follow this course is to think of the System as having a single instrument variable—the maximum of earning assets which member banks as a group can hold—instead of three.

This is not the place to define the maximum earning assets variable in an operational way. For present purposes it is sufficient to note that it is a function of (1) the average member bank reserve requirement, (2) the total of member bank reserves, and (3) the distribution of total reserves over all possible sources of reserves.[1] When the average member bank reserve requirement changes, so of course does member banks' capacity to carry earning assets. When total member bank reserves changes, this capacity is similarly altered. But exactly how much this capacity is altered by a change in total reserves depends on the composition of this change, on the distribution of the gain or loss as between the several sources of reserves. For example, reserves obtained by discounting will support a greater volume of loans than will an equal volume of reserves obtained from, say, a gold inflow.

That this maximum earning assets variable can be considered an instrument variable of the Federal Reserve seems clear. Its value at a particular instant in time is independent of member banks' portfolio preferences. And, although the System probably cannot control the ability of member banks to hold earning assets from moment to moment, it probably can come tolerably close to some preassigned (average) value if given a week or a month or longer. To be sure, total reserves of member banks is the sum of several variables, not all of which are System instrument variables. But System holdings of Treasury securities, which is one of the variables determining total reserves, is an instrument variable; and if moment-to-moment control, which seems unnecessary, is not insisted upon, this is sufficient to make the total of member bank reserves itself an instrument variable. Also, the average member bank reserve requirement, though influenced by changes in the distribution of time and demand deposits, should be thought of as an instrument variable of the System; and this average reserve requirement, it will be recalled, enters as one of the determinants of the maximum of earning assets which member banks as a group can hold.[2]

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[1] If there is an irreducible minimum of vault cash which member banks must hold, then maximum earning assets must be considered as depending on the variables which determine this minimum. (This minimum, incidentally, may be considered as a technological constraint.) The significance of this minimum vault cash variable is not sufficient, however, to justify the space necessary to consider whether a minimum exists, and, if it does, how it enters as a determinant of maximum earning assets.

[2] It should be noted that observed total reserves can be transformed into other variables which have the property of describing the classical portion of Federal Reserve strategy; namely, maximum money supply and effective reserves. Horwich [8] has computed a time series of effective reserves and so has Warburton [17]. See also [3] and [4], for in these papers explicit attention is given to the System's instrument variables.

To consider maximum earning assets as the instrument variable of the Federal Reserve—to offer a description of it as a description of classical monetary strategy—is to put near-exclusive emphasis on the supply of excess reserves. (First differences of this variable measure the incremental supply of excess reserves to member banks.) Some students of monetary matters will surely object, though, to the idea that the overwhelmingly most important aspect of Federal Reserve control is the control of the supply of excess reserves. What of the "announcement effects" associated with changes in discount rates and reserve requirements? Nor can it be denied that to describe the course of System strategy by means of one rather than three time series is to ignore certain information. There are, after all, many combinations of values for discount rates, reserve requirements, and System holdings of Treasury securities which will produce a particular value for maximum earning assets. And if it really does make a difference which combination of values for the classical instrument variables gives rise to a particular value of the maximum earning assets variable, then of course the latter fails as an adequate description of System strategy.

The adequacy of a single variable, or one-dimensional, description of Federal Reserve strategy can only be determined empirically; the results it yields must be compared with those yielded by alternative descriptions. So far, however, this has not been done. But it should be noted that even if some information is lost in the single variable approach, it can still rank superior to alternative approaches. From a purely statistical point of view, there sometimes is an advantage apparently in having one rather than three instrument variables. Then, too, implicit in the use of maximum earning assets to describe Federal Reserve strategy is a solution to a vexing weighting or index number problem—a solution, moreover, which does not involve an arbitrary assignment of weights. Without some way of combining the specific values assumed by the three classic instrument variables in any given period, it is not always possible to make intertemporal comparisons of Federal Reserve policy. (Does a 3 per cent discount rate and a System portfolio of Treasury securities worth 20 billion dollars represent a tighter monetary policy than a 2.5 per cent discount rate and a System portfolio worth 19 billion?) When a single instrument variable is used to describe System strategy, though, there can never be any question about how that of one period compares with that of another.

The Federal Reserve and Credit Supply Conditions

In attempting to sketch the case for a one-dimensional description of Federal Reserve strategy, I have, I believe, reminded us of a rather important point: that neither the money supply, however defined, nor

any measure of interest rates, can be taken as an instrument variable. Sometimes this is just what is done; Keynes, for example, in his *General Theory*, took the money supply as the instrument variable of the monetary authority, and often present-day economists, whether inclined toward the Keynesian or quantity theory approach, do the same, particularly when outlining how monetary policy works. But to follow this course is to emphasize one aspect of monetary policy, the relationship between credit conditions (or terms) and spending decisions, at the expense of another, the relationship between maximum earning assets and supply conditions in the several markets for debt and equity contracts.[3] This latter relationship, though, is of no little importance.

Certainly if classical monetary policy is to be of use in fighting severe economic contractions, if it is to have complete two-way effectiveness, then member banks' demand for excess reserves, no less than the public's demand for money, must be free of a liquidity trap. Of this, everyone is fully aware. But the available empirical evidence does not, I think, permit us to be even moderately confident about whether a hypothesized liquidity trap in member banks' demand for excess reserves is fact or fiction.

Lately there seems to have developed a tendency to interpret monetary developments of 1936-37 as showing the absence of a liquidity trap in member banks' demand for excess reserves. The Federal Reserve, it will be recalled, having just obtained the power to vary reserve requirements, increased them three times during 1936-37. And these increases were followed by increases in interest rates.[4] Of course, to discover the significance of this sequence of events for the existence of a liquidity trap in member banks' demand for excess reserves, it is necessary to take account of possible independent increases in the demand for credit. Since the economy was expanding in 1936, this possibility is not unreasonable. But there is another, more interesting, difficulty standing between the events of 1936-37, mentioned above, and the obvious interpretation of them. Underlying the view that events of 1936-37 contradict the liquidity trap hypothesis there must be a feeling that if this hypothesis is not to be considered pointless, then it can reasonably be assumed that member banks were, in a manner of speaking, operating far (or sufficiently far) out on the horizontal por-

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[3] Those who prefer that the working of monetary policy be phrased in terms of the money supply can think of this latter relationship as being between the maximum money supply, which, as already noted, can be considered a System instrument variable, and the actual money supply, which presumably is related to the positions of the several supplies of different types of loanable funds.

[4] Both Samuelson [11] and Friedman [5] have suggested that the System officials responsible for the 1936-37 increases in reserve requirements were guided by an excessively naïve Keyensian view of liquidity preference. On the wisdom of these increases, see [3].

tion of their demand curve. In this connection, however, it is interesting
to note that a scatter diagram relating the interest rate on four- to
six-month commercial paper and the ratio, for all member banks, of
primary reserves[5] to total assets less required reserves shows the points
for 1935 and 1936 lying approximately where the derivative (with
respect to the interest rate) of a curve drawn through all points changes
from finite to infinite.[6] Perhaps, then, member banks were operating
at the "kink" in their demand curve just before reserve requirements
were increased. (Thus, from the viewpoint of testing the liquidity trap
hypothesis, a reduction in reserve requirements during 1936-37 would
have been better than the increase which actually took place.)

Of course, much more needs to be known about member banks'
demand for excess reserves than whether it becomes infinitely interest
elastic at some level (or structure) of interest rates. Presumably, the
Federal Reserve must have a pretty good idea of what this demand
looks like in its entirety. Without this information, the System cannot
hope to exert precise control over its target variables. The trial-and-
error approach, if sometimes inevitable, is no speedier for that; and it
may involve unnecessary, and possibly harmful, fluctuations in mem-
ber banks' reserve positions. Nor can System officials, or anyone else,
hope to decide the feasibility of the nondiscretionary monetary policy
advocated by Friedman [5] and Shaw [14] without this information.

If guided by Friedman and Shaw, the Federal Reserve would seek
to increase the money supply, however defined, at a constant rate from
month to month and year to year. That it would inevitably fail if it
came up against a liquidity trap in member banks' demand for excess
reserves is apparent. That it might fail even in the absence of a liquidity
trap is a possibility which, if less apparent, must nevertheless be given
some thought. Imagine that the System comes up against a desire on
the part of member banks to retain as excess reserves not quite but
nearly 100 per cent of all excess reserves supplied them. To press for
a given increase in the money supply in the face of this desire, however,
is also to build up a large stock of excess reserves. And in building up
this stock, the Federal Reserve may defeat itself; perverse expecta-
tional effects are not unknown in the economic world. Then, too, the
Federal Reserve may be storing up future trouble for itself; official
ruminations about monetary policy during the period 1953-56, and
particularly 1953-54, seem to suggest a belief in an upper limit on the
rate at which previously created excess reserves can be destroyed.

No one, it seems to me, can say whether these possibilities are real

[5] Primary reserves is the sum of excess reserves, vault cash, and balances due from
banks.
[6] The scatter diagram referred to here appears in [10].

or imaginary. But, then, if I cannot say definitely that actual conditions will confound the Friedman-Shaw approach, neither can its proponents confidently assert the opposite.[7]

Although member banks' demand for excess reserves is undoubtedly the single most important aspect of their (collective) portfolio preferences, a case can nevertheless be made, I believe, for the need to know how changes in the System's instrument variable, maximum earning assets, affects their willingness to supply each of the several types of loanable funds in which they customarily deal. This is just the sort of information which is required for an intelligent decision about how consistent (whether absolutely or as compared with other stabilization approaches) the use of monetary policy is with national objectives other than price-level stability and full employment. Moreover, it may well be that the relative efficiency of monetary policy, as a stabilization technique, cannot really be appraised without this information.

An idea of the relative efficiency of alternative stabilization techniques can be got, at least when inflation is the problem, by comparing the amounts of unemployment generated by each of the alternatives when they are pushed to the point where, by themselves, they yield price stability. True, this notion of relative efficiency is without meaning for an economy in which all prices move promptly and fully in response to supply-demand imbalances and all labor is perfectly mobile, or nearly so.[8] But these are strong, oft-challenged assumptions. Only recently they were challenged by Schultze [13] and by Eckstein [19]. And to relax these assumptions is to raise doubts about the appropriateness of monetary policy for all inflationary situations. If prices are rigid downward and labor is immoble and if the excess demand sectors of the economy are not sensitive to changes in the monetary climate, then the use of monetary policy can produce considerable unemployment, perhaps more than is necessary.

Brownlee and Conrad, in their study of the income redistributive effects of alternative stabilization techniques [2], have made a start toward determining the relative efficiency of these techniques. Using their own estimates of sector man-hour requirements, the 1947 estimates of the Leontieff coefficients, and the 1957 structure of final demands, they have attempted to determine, along with other things, the unem-

[7] Friedman [5] grants, though only as a theoretical possibility, that trouble can arise out of the fact that the System controls not the actual money supply but rather the maximum money supply. To my knowledge, however, he has not presented any empirical evidence on the above points. There are available two good studies of the extent to which, and the speed with which, member banks respond to inputs of excess reserves; namely, [1] and [8]. But neither study covers more than a few years, nor more than a fairly narrow range of economic circumstances.

[8] The assumption that inflationary pressures are always, or nearly always, uniformily distributed over the entire economy also makes this notion of relative efficiency pretty much pointless.

ployment associated with monetary, tax, and expenditure policies and have, I believe, come up with some interesting (albeit tentative) differences. Seemingly, though, their estimates are based on some extreme assumptions, as, for example, that labor is perfectly immobile. Of course, it is not obvious how the perfect immobility assumption can be relaxed; nor is it obvious how better estimates of industry (or sector) interest elasticities can be obtained. But that additional efforts should be made along these lines seems reasonable. Also, the Brownlee-Conrad estimates are available, as was indicated, only for one structure of final demands (1957), and what we should want to know is how the efficiency ratings of the alternative stabilization techniques vary with changes in the structure of final demands.

So far I have mentioned only the willingness of commercial banks to lend in the several loanable funds markets. But nonbank financial intermediaries and firms also operate in most of these markets. Apparently, then, the question posed above (how do changes in the System's instrument variable influence different sectors of the economy) cannot be given a reasonably complete answer without some knowledge of the behavior patterns of nonbank lenders. Nor, therefore, can the demands of the public for the liabilities of different lenders—most importantly, of bank and nonbank intermediaries—be ignored. It is rather hard to tell from the Radcliffe *Report* [18] itself, but a recent postscript by Sayers [12], himself a member of the Radcliffe Committee, makes it clear that the roles played by the public, and more, by financial intermediaries, are what the Committee meant to emphasize.[9] Not that the introduction of financial intermediaries forces us to modify our theoretical (comparative statical) conclusions about the effectiveness of classical monetary policy. In the real world, however, and in dynamic models as well, where lags can be important and errors of timing of significance, the rise of financial intermediaries appears as a vexing complication which must be studied further.

There is finally one other rather neglected aspect of member banks' behavior to which I should like to call attention; namely, the rationing of credit by non-price means. I do not mean that this possibility has not been recognized. Quite the contrary. In his *Treatise on Money*, Keynes wrote of an "unsatisfied fringe of borrowers." And since the end of World War II we have heard increasingly of the "availability of credit," which must, I think, be read "the degree of non-price ration-

[9] The Radcliffe *Report* contributes little, I think, to our empirical knowledge of monetary policy, although it does contain many hitherto unavailable numbers which will help future researchers. But like the Eckstein *Report* [19], the Radcliffe *Report* does call attention to an important point, and this is more than enough to make it a worth-while document.

ing."[10] But so far there have been no systematic studies of how lenders, whether bank or nonbank, vary credit standards in response to changes in the Federal Reserve's instrument variable or other aspects of their environment.

That member banks, or lenders generally, may alter credit standards as their situations change seems quite plausible a priori. They operate, after all, in a risky (or uncertain) world, and to ration by non-price means is to control the risks being run in lending. And there would seem to be good reasons for investigating this possibility that lenders alter credit standards over, say, the cycle. It would seem to have definite welfare implications; and it bears directly on the question of direct controls. Also, to estimate interest elasticities of credit demands, it may be necessary to know something about how lenders credit standards change (if they do). If we could be sure that these standards are rigidly linked to interest rates, this knowledge might not be so important. But of course we cannot know this until we have actually studied the behavior of credit terms.

Determining how member banks and other lenders alter credit terms in response to changes in the Federal Reserve's instrument variable will not be easy, at least in the near future, for the required data are not available. In general, it is not possible to infer from market determined values how lenders alter their credit standards. For instance, the average maturity on consumer loans made in a particular period is determined partly by relative demands for loans of different maturities. What is wanted, then, are time series of lenders terms—say, the maximum maturity for a certain income class, or the minimum quick asset ratio necessary to qualify for a specific type of business loan—and these, as I have indicated, are not yet available.

An Optimistic Postscript

If in the foregoing I have created the impression that our knowledge of monetary policy if judged by what we should know is on the slight side, let me hastily add that it is perhaps even slighter than I have made it seem. There are many problem areas which have gone unmentioned. Nothing has been said of the sensitivity of borrowing and spending to changes in interest rates and non-price variables. Nor has the problem of lags, the study of which is only beginning [5, 9, 15], been sufficiently emphasized. But I should also add that the prospect for a rapid increase in our knowledge of monetary policy is bright. The Commission on Money and Credit, if it does not tell us all we need to know, will cer-

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[10] For the proper interpretation of this phrase "credit availability," see the excellent papers by Guttentag [6] and Hodgman [7].

tainly stimulate much more research. (Incidently, to judge from the lists of Ph.D. theses under way, interest in empirical studies of monetary policy is already on the increase.) Then, too, econometric methods, though new as methods go, are beginning to be used in the money and banking area. This is an especially good sign, since for years now this area has been something of a backwater.

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SOME MAJOR PROBLEMS IN MONETARY THEORY*

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I

Monetary policy operates directly on the Federal Reserve's portfolio of government securities, the requirement ratios, and the rediscount rate. The transmission of the desired effects to the target variables (income, employment, and prices) presupposes a systematic connection between policy and target variables crucially mediated by a set of monetary magnitudes. It therefore appears convenient to subdivide the monetary mechanisms into two branches: one relates policy and monetary variables and the other associates monetary variables with income or prices. Money supply theory explicates the first subrelation and money demand theory together with aggregate demand theory defines the second subrelation. Existence and nature of these subrelations form the central issue of recent policy discussions. An evaluation of the degree of effectiveness of monetary policy consequently involves a comparative appraisal of rival theories concerning the properties of the two subrelations. Such appraisal has barely begun, particularly as many conceptions advanced still require a translation into properly formulated hypotheses. This situation explains the emergence of abundant references to observable patterns which actually possess no evidential significance or discriminating power but are diligently adduced to support contentions concerning the usefulness of monetary policy.[1] Analysis of these patterns indicates that they are equally consistent with alternative hypotheses which imply opposite statements about the effectiveness of monetary policy. Explication of vague conceptions into empirically significant theories thus forms a necessary step in the resolution of conflicting ideas. The construction and comparative assessment of such theories alone assures "cognitive respectability" to policy discussions.

[1] Three observation statements, one referring to the relative growth of nonbanking financial intermediaries, another referring to large or increasing excess reserves in a deflationary environment and a third to a systematic association between restrictive policy and rising velocity, whose truth we may concede, have been advanced in support of the contention that monetary policy is decreasingly effective. The fallacy involved is revealed by constructing a hypothesis which implies the three observation statements together with a statement about an unreduced or even increasing degree of effectiveness for policy actions. Logical analysis thus exhibits the three observation statements to be consistent with a theory which implies continued usefulness of monetary policy.

II

Long neglected, money supply theory has recently attracted more attention, and a variety of promising leads have been developed. A rough outline of a potentially fruitful formulation follows.

The institutional arrangements of our financial system suggest that the money stock, assets, and liabilities of all financial institutions are jointly determined by the operation of the credit markets. The banks' net flow demand for earning assets is determined by the banks' wealth (balance-sheet) position and pertinent market prices; i.e., an index of interest rates formed on the bank oriented credit market. The public's net flow supply of assets to the banks depends on the public's wealth position, the index rate, interest rates on related credit markets, and current income. The equilibrium of net demand and supply determines the index rate as a function of the public's and the banks' wealth position, interest on other credit markets, and income. This flow equilibrium is consistent with continuous changes in the banks' wealth position, in particular with changes in the portfolio of earning assets. A determination of this stock magnitude is obtained with a condition of stock equilibrium, equating the banks' net flow demand to zero. The banks' net borrowing from the Federal Reserve banks is explained by a flow demand function with balance-sheet position, index rate, and rediscount rate as arguments. The optimal stock of outstanding discounts and advances to commercial banks is again determined by a condition of stock equilibrium equating net borrowing to zero. All equilibrium conditions, stock or flow, can be justified in terms of a rapid adjustment of interest rates and bank positions relative to time units implicit in the definition of observable magnitudes associated with the hypothesis. More significant is the cognitive function of such equilibrium conditions: They imply that variations in exogenous variables are a necessary condition for the occurrence of variations in endogenous variables.

The construction is completed by specifying the processes changing the banks' volume of "free" cash assets. Money stock, the banks' portfolio of earning assets, the index rate, the volume of reserves, the volume of excess reserves, and the amount of indebtedness to Federal Reserve banks are jointly explained by this formulation in terms of the (adjusted) monetary base[2] plus the cumulated sum of reserves liberated (or frozen in) by past changes in the requirement ratios, the rediscount rate, two parameters expressing specific asset preferences

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[2] The adjusted monetary base is equal to the monetary base minus discounts and advances. The monetary base is the amount of money directly issued by the authorities. Its precise definition depends on the institutional arrangements.

of the public, an index of interest rates on related credit markets, the outstanding stock of government securities, and income.[3]

The "money supply function" is thus obtained as a solution of the formal structure describing the operation of the bank oriented credit market. The function defines a relation between policy variables and the money stock. Partial correlation analysis based on monthly data or on quarterly averages of monthly data and covering very differently situated sample periods confirms this connection between the monetary base, the reserve requirements, and the money supply. It appears that the monetary base is the most important magnitude explaining the behavior of the money stock. Explanations which disregard the base yield thoroughly unreliable results or factually erroneous conclusions.[4] The relative importance of the base does not signify irrelevance of other explanatory magnitudes. Variations in reserve requirements contribute substantially to the behavior of the money stock as do the shifts in parameters expressing the public's asset preferences between currency and deposits, or between demand and time deposits.

A useful operation of monetary policy depends on the effective transmission of policy actions to the money stock and, conceivably, to other monetary variables. Existence or absence of such a transmission, particularly in a deflationary environment, is still under consideration. The hypothesis outlined enables a systematic inquiry into crucial links of the monetary mechanism which endanger a persistent connection between policy and monetary variables. With a vanishing interest elasticity of the public's net supply of assets to banks, a vanishing elasticity of the banks' net demand for assets with respect to an accrual of excess reserves, and an indefinitely large interest elasticity of the banks' asset acquisition, the monetary variables would respond to neither open market nor to requirement policy. Experience indicates that a direct evaluation of the public's and the banks' behavior properties yields, at best, tenuous and unclear evidence. The hypothesis eliminates this difficulty by a transformation of not directly assayable propositions into statements to which we may associate a meaningful appraisal procedure. The hypothesis implies that each of the three specified elasticities is a sufficient condition for the money supply function to have a derivative not exceeding unity with respect to the base and a zero de-

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[3] See Appendix A for a concise formal statement, together with a few observational results.

[4] The Radcliffe *Report* apparently misses the significance of the base. The report complains that the money supply grew during the last decade in spite of a constant cash ratio and concludes that the money supply was evidently uncontrolled. The report neglects completely that the base is the most important determinant of the money stock and that the base grew continuously and at an accelerating rate since 1949 by courtesy of U.K. policy.

rivative with respect to the requirement ratios and with respect to the parameters expressing the public's pertinent asset preferences. Consequently, any evidence bearing directly on the properties of the money supply function contributes indirectly to assay propositions about the crucial elasticities in the structure of the bank oriented credit market. Estimates derived from observations generated under radically different economic conditions consistently yield values of the "monetary multiplier" (i.e., the derivative of the money supply function with respect to the base) in the range 1.5 to 3.5 and confirm the significance, expected sign, and expected relative order of magnitude of other derivatives. The persistent pattern of the results obtained under deflationary and inflationary "economic climates" is incompatible with the indicated behavior elasticities blocking the transmission of policy actions to the monetary variables.

Discussions about monetary policy frequently assign to bank reserves a crucial significance in the money supply mechanism. The precise meaning of these statements is often ambiguous, and their formulation permits the following three alternative explications: the volume of bank reserves is a policy variable; the volume of bank reserves is immediately and completely controlled by policy variables; and the volume of bank reserves is a target variable; it is chosen by the authorities as a signal for appropriate policy actions. The first interpretation can be immediately dismissed. The third interpretation admits bank reserves as an endogenous magnitude determined by the total interaction of all pertinent relations. The important issue under this interpretation bears on the rational choice of signs by the monetary authorities, and I contend that to include bank reserves among the signs to be watched raises the likelihood of inappropriate actions, measured in terms of income stabilization. The second interpretation raises a substantive issue. It usually involves a dismissal of the public's asset preferences as a determinant of money supply behavior and, in particular, a dismissal of the public's marginal propensity to hold currency. Its logical structure appears to exhibit a causal ordering of money stock, reserves, and monetary base which conflicts with the causal ordering determined by the theory outlined. The second interpretation implies a linear ordering from B over R to M; whereas, the theory specifies both M and R as jointly dependent on B. This latter formulation permits an evaluation of the issue with the aid of partial correlation analysis, and we observe that the results are more compatible with the theory outlined than with the second interpretation.[5]

[5] The partial Kendall coefficients for the period 1947-57, computed from quarterly averages of seasonally unadjusted monthly values, are:

$$M, B/R = +.581 \qquad\qquad M, R/B = .058$$

III

An effective connection between policy variables and monetary variables is a necessary but not a sufficient condition for the transmission of policy actions to the relevant target variables. A useful operation of monetary policy requires, in addition, an effective causal connection between monetary and target variables. Observations of high correlations between income or prices and the money stock affirm the significant existence of the second subrelation. However, this correlation conveys no information about the causal ordering of the underlying structure which generates the relation. We are yet confronted by rival interpretations of this relation. One interpretation recognizes, in the observed correlation, the demand for money and denies the existence of any significant feedback from monetary variables to aggregate demand for output. Another interpretation explains the second subrelation in terms of the joint interaction of a "demand pull" for money and an "asset push" on aggregate demand. Under the first interpretation monetary policy would be useless, unless we acknowledge a substantial interest elasticity of aggregate demand; whereas, the second interpretation provides a theoretical basis for policy actions.

Both interpretations admit the notion of a demand function for money, and the suitable formulation of the substantive issue concerning the causal nexus of the monetary variables requires explicit consideration of this demand function. The Keynesian analysis generalized the Cambridge demand schedule by relating the desired money balance to both interest rate and current income. A demand function of this type can be derived from a number of higher level hypotheses. Such analytical formulation may exhibit the theory under consideration as a connected part of a more general structure, but it cannot justify the theory's empirical content. Quarterly observations drawn from the period 1939-57 and annual observations covering the period 1929-59 yield coefficient estimates which are statistically significant and also possess the expected sign. The results thus confirm the basic idea, expressed by the Keynesian demand function, viz., that desired balances are systematically associated with interest rates and a major component of current transactions. Purely "associative laws" form a considerable part of our systematic knowledge, and they contribute usefully to elucidate a number of broad policy problems. Nevertheless, the usefulness of a theory rises with our ability to specify relatively stable orders of magnitude.

It should be noted that the hypothesis outlined can be extended to cover related credit markets and thus to incorporate the behavior patterns of nonbanking financial institutions. A preliminary investigation of the comparative usefulness of the hypothesis yielded an average error of 1.8 per cent in predicting ten observations not contained in the sample underlying the coefficient estimates.

Stability of the quantitative properties permits a successful application of the hypothesis to more detailed policy situations. Unfortunately, the demand hypothesis under consideration does not satisfy these more stringent requirements.

A diligent search for more fruitful hypotheses is under way. Promising modifications are suggested by an errant tradition which relates desired balances primarily to "wealth" and interest rates. The success of these modifications depends decisively on a useful specification of the term wealth. One line of investigations, opened by Professor Friedman, determines an index of wealth with the aid of a functional containing current income's time function as an argument. This demand theory explains the desired balances in terms of the wealth index per capita (labeled "permanent" income), population and permanent prices. The Friedman hypothesis yields an explanation of velocity which consistently subsumes both secular and cyclic behavior and to this extent, at least, appears more highly corroborated than the Keynesian demand function. Yet, the demand theory formulated by Friedman has to prove its mettle under a more detailed quantitative appraisal. A casual inspection of the data, particularly for the thirties and fifties, reveals quite substantial differences between actual and estimated velocities, and the signs of the deviations seem broadly related to the comparative levels of interest rates in the two periods. Two modifications of the Friedman theory were therefore tentatively considered, both of which maintain the advance gained in the explanation of velocity. Each modification incorporates an index of interest rates; one replaces permanent prices by current price, and the other replaces permanent prices by transitory income in current prices. Estimates were based on annual data drawn from the period 1919 to 1959 and computed for a number of subperiods. With one exception, having a very small power of discrimination, the interest elasticities are significantly negative; the elasticity of desired balances with respect to "permanent income" dominates persistently; its value exceeds considerably the value of interest elasticity and is approximately three times the elasticity with respect to current prices or transitory income.[6]

The concept of a demand for money helps clarify the nature of the chain linking monetary variables and current output. The demand function for money reveals an aspect of the public's allocation pattern for wealth. The optimal composition is determined by the "inherited"

[6] The modification of the Friedman hypothesis involving current prices was estimated for the total period and four subperiods. Only the subperiod 1919-34 yielded a positive and nonsignificant interest elasticity. The modification involving transitory income was estimated for the total period and two subperiods. The estimated interest elasticities are all significantly negative and cluster closely in the range —.27 to —.22. See furthermore Appendix B.

portfolio, the price-constellation, and the preferences between types of assets and liabilities. Variations in prices and the inherited position induce, in general, readjustments in the optimal portfolio. These re-arrangements and the associated price movements are an essential feature of the monetary mechanism which transmits changes in policy variables to national income or current output. Deviations of actual from desired balances induce readjustments in the public's balance-sheet position involving the whole range of assets and liabilities. These re-adjustments in asset portfolios spill over into the markets for current output. Three links of the chain are of particular significance: bonds are not the only substitute for money, for substitutability permeates the whole spectrum of assets, production of assets is a close substitute for the existing assets, and many services are close substitutes for the holding of assets. An increase in the public's money balances generated by suitable policy actions thus triggers a substitution chain in the public's portfolio which spills over to new production of assets and services and thus affects aggregate demand for output.

This general argument can be represented by an extensive class of formal structures. These formalizations explicate the idea that optimal stocks and flows (i.e., purchases) depend simultaneously on prices and wealth. The money stock affects the demand decisions for assets and output as a component of the inherited wealth position, and the useful-ness of monetary policy hinges on the circumstance that such action simultaneously modifies the market situation confronting the public and its inherited portfolio. The precise incorporation of wealth into the demand function, particularly into the components of aggregate de-mand for output, still requires investigation. One view holds that net worth completely summarizes the relation between a portfolio position and demand behavior. This view implies that variations in the compo-sition of a constant net worth exert no effect on demand. Another view contends that both net worth and its composition affect demand be-havior. A particularly important aspect of this problem concerns the comparative order of asset and liability effects. The net worth hy-pothesis, for instance, assigns equal significance to both. The resolution of this issue has far-reaching ramifications. Our interpretation of non-banking financial intermediaries, of changes in their portfolio composi-tion exemplified by a loan expansion balanced by a sale of government securities, and of new issues of government securities depends sub-stantially on the existence and order of magnitude of the liability effects. The degree of inflation resulting from a reallocation of resources to the government financed by an injection of base money is independent, un-der a net worth hypothesis, of the monetary multiplier. This multiplier

would be significant in case the asset effects exceed the liability effects, and demand behavior depends on the composition of net worth.

The general idea of a balance-sheet reaction process with eventual spill-over to current production of assets and services appears to explain without difficulties a number of observable patterns. A broad range of observations is consistent with hypotheses based on this idea and difficult to reconcile with theories which neglect or explicitly deny the chain of interdependent balance-sheet adjustments. Among the pertinent observations we may note the following four: A constant deficit financed by new issues of base money has been associated without exception with a rising price level, and stabilization occurred only when the relation between deficit and the rate of change of the base was broken. The elimination of controls after a large accrual of money balances permits a delayed adjustment of actual to desired wealth positions, and the usually occurring increase in prices reveals the operation of this process. Furthermore, cross-section data seem to indicate that money balances are not erratically distributed among economic units relative to other components of wealth. In particular, larger balances are associated, in the average, with greater values of most other important types of assets and liabilities. Lastly, the operation of the portfolio adjustment mechanism implies that, at least in periods exhibiting sufficiently large variations in the money stock, hypotheses which incorporate this monetary variable as an argument of the aggregate demand function for output yield better results than hypotheses which eliminate any reference to monetary variables.[7] These remarks do not justify a particular class of monetary theories—but they do constitute a case for considerable investment of resources to further a detailed investigation of the chain connecting monetary and target variables.

APPENDIX

A. The money supply hypothesis is characterized by the following variables and relations: $M =$ money stock (including time deposits); $b =$ monetary base; $E =$ banks' portfolio of earning assets; $k =$ rate at which reserves are liberated or frozen in by current changes in reserve requirements; $K =$ integral over time of k; $v =$ volume of excess reserves; $i_1 =$ index of bank loan-rates and bond yields; $i_2 =$ index of interest rates on related credit markets; $d =$ rediscount rate; $n =$ banks' volume of indebtedness to Federal Reserve; $S =$ government securities outside government sector (including central bank); $Y =$ national income; $C^p =$ currency held by public; $T =$ time deposits held by public. The equations are:

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[7] Some evidence bearing on this problem was presented in the paper by K. Brunner and A. Balbach, "An Evaluation of Two Types of Monetary Theories," *Proceedings of the Western Econ. Asso.*, 1959.

(1) $\dot{E}^i = h(i_1, d, v, E, n)$; $h_1 > 0 > h_2$; $h_3 > 0 > h_4$; $h_5 < 0$
banks' net flow demand for earning assets.

(2) $\dot{E}^* = f(i_1, i_2, E, S, Y)$; $f_1 < 0 < f_2$; $f_3 < 0 > f_4$; $f_5 > 0$
public's net flow supply of assets to banks.

(3) $\dot{n} = g(i_1, d, v, E, n)$; $g_1 > 0 > g_2$; $g_3 < 0 < g_4$; $g_5 < 0$
borrowing from Federal Reserve by commercial banks

(4) $C^p = c_0(z) + .15M$
public's demand for currency.
z is an unspecified vector, not immediately needed for our purposes.
The analysis uses c_0, the demand component independent of the
monetary wealth.

(5) $T = t_0(y) + .15M$ public's demand for time deposits.
y is an unspecified vector. t_0 is the demand component independent
of monetary wealth.

(6) $\dot{v} = a_1 b + k - a_2 c_0 + a_3 t_0 - a_4 \dot{E}$; accrual of excess reserves; $0 < a_i < 1$
for every i.
The detailed nature of the coefficients a_i is determined by the in-
stitutional arrangements of the system.

(7) $h = f$ condition of flow equilibrium

(8) $h = 0$ condition of stock equilibrium

(9) $M = b + E$ definition of money supply

(10) $g = 0$ condition of stock equilibrium for n

A linear approximation to the solution for the money supply can be
obtained from the above system:

$$M = m_0 + m_1 b + m_2 K - m_3 c_0 + m_4 t_0 + m_5 Y - m_6 S + m_7 i_2$$

$$m_1 \sim 1 + m_2; \qquad m_1 \sim m_3 > m_4 > m_5 \sim m_6 > 0$$

It seems that as a first approximation we may often neglect the last three
terms, particularly in case they exhibit offsetting movements. Quarterly
data from the period II/1929–IV/1933 were used to compute one such
approximation. The regression estimated from the sample period 1929-
40 (annual data) reveals the order of magnitude pattern of the coeffi-
cients. The regressions were deliberately selected from the most defla-
tionary environment of recent experience. Sample period II/1929-
IV/1933:

$$M = 9.07 + 1.86(b + K) - 2.75c_0 + 1.11t_0$$

$$R = .997 \quad (.83) \qquad\qquad (.49) \qquad (.24)$$

$$.50 \qquad\qquad -.83 \qquad .77$$

Sample period 1929-40:

$$M = 8.93 + 1.50b + .23K - 2.12c_0 + 1.11t_0 + .06Y - .09S$$

$$R = .995 \quad (.12) \quad (.24) \quad (.56) \quad (.24) \quad (.02) \quad (.02)$$

$$.90 \quad .15 \quad -.51 \quad .59 \quad .43 \quad -.53$$

The numbers in parenthesis below the regression coefficients are standard errors. R indicates the multiple correlation coefficient and the second row of numbers below the regression coefficients are the partial correlation coefficient.

B. The three types of demand functions yielded the following estimates for their respective total sample period:

(1) sample period 1929-55 (annual data):

$$\log \frac{M}{N} = -.30 - .23 \log i + .89 \log \frac{Y}{N}$$

$$R = .990 \qquad (.06) \qquad (.03)$$
$$\qquad\qquad\qquad -.64 \qquad\quad .97$$

(2) sample period 1929-55 (annual data):

$$\log \frac{M}{N} = -2.06 - .22 \log i + 1.53 \log w + .57 \log \frac{y}{w} + .57 \log p$$

$$R = .990 \quad (.07) \qquad (.59) \qquad (.15) \qquad\qquad (.15)$$

(3) sample period 1919-55 (annual data):

$$\log \frac{M}{N} = -3.45 - .30 \log i + 1.70 \log w + .48 \log P$$

$$R = .993 \qquad (.05) \qquad (.12) \qquad (.06)$$
$$\qquad\qquad\qquad -.73 \qquad -.93 \qquad\quad .82$$

Numbers in parenthesis are standard errors. M = money stock (including time deposits); N = population; i = index of bank loan rates and bond yields; w = "permanent" income per capita; Y = GNP in current prices; y = GNP per capita in constant prices; p = implicit GNP deflator; P = index of wholesale prices.

Equation (b) is obtained by rearranging the original regression which contains $\log i$, $\log w$, and $(\log Y/N - \log w)$ as arguments. R = multiple correlation coefficient. Second row of numbers below regression states the partial correlation coefficients.

DISCUSSION

THOMAS MAYER: Professor Kareken has discussed a number of areas of our ignorance about monetary policy with emphasis upon the technical problems of the central bank's management of the reserve base. I have little quarrel with Professor Kareken's statement that we have insufficient knowledge of these subjects—indeed it would be hard to disagree with such a statement about almost any topic in economics. However, I do differ with him on the relative emphasis he seems to place on different areas.

There are five things the Federal Reserve should know. The first is the goal, or combined goals, to aim for. The second is future business conditions. The third is the speed of its policy, the fourth is the change in aggregate demand per dollar change in the reserve base, and fifth is the advantages and disadvantages which monetary policy has apart from its effects on stabilization.

On the first of these points—the goal to aim for—we have made but little progress. Not only do we hold different value judgments regarding the relative importance of different goals, but we also do not know how much of one we have to sacrifice to obtain a given amount of the other. Moreover, to use monetary policy effectively we would have to know, not only one particular "trade-off" ratio, but we would have to know the most efficient ratio. In other words, we would have to be able to answer a question such as: suppose that we are willing to pay the unemployment cost of price stability, would it be more efficient to tolerate a considerable volume of long-run unemployment, but not a serious depression, or would we be better off having little secular unemployment, but allowing an occasional sharp depression to teach sellers a "lesson"?

Turning to forecasting, the Klein-Goldberger model so far has forecast quite well, but we do not know whether it is accurate enough to serve the purposes of monetary policy. Another alternative would be to forecast by changes in the money stock. If this method of forecasting is better than the Klein-Goldberger model, or other Keynesian models, then this has a very interesting implication—for this would suggest that increasing the money stock at a constant rate as suggested by Friedman and Shaw may well be preferable to discretionary action. A forthcoming paper by Bronfenbrenner suggests that this may well be the case. Next, there is the problem of the speed of monetary policy. According to Friedman there is substantial variation in the time required by monetary policy to become effective; if so, even given an accurate forecast of business conditions, monetary policy may be destabilizing.

The next problem is predicting the magnitude of the effect of Federal Reserve action. This is the main problem discussed by Professor Kareken. I am less concerned than he is about banks piling up excess reserves, since, in the postwar period, excess reserves have remained low and stable. In a deep depression we may, of course, run into a liquidity trap. Whether this would actually happen is hard to say, because one cannot generalize from one case,

the thirties. Moreover, even if this does happen, the problem may not be as serious as Professor Kareken suggests, for although we have more idle reserves to mop up during the subsequent recovery, we have more time to do it in, because recoveries from deep depressions take longer to become inflationary (rather than reflationary) than do recoveries from mild recessions. In other words, during a severe depression the Federal Reserve does not have to worry that the currently induced increase in the money supply will become effective during the next boom, and hence be destabilizing. I am therefore somewhat skeptical of the frequent statement that monetary policy is clearly a more effective stabilization tool during the boom than during a severe depression, for this statement appears to emphasize the problem of strength relative to the problem of correct timing.

A problem related to the excess reserve problem is the possibility of velocity shifts, for, as Warren Smith has shown, in the very process of minimizing their excess reserves through security purchases, banks may reduce velocity. Lawrence Ritter has argued that such movements of velocity can be allowed for by using a stronger monetary policy than one otherwise would. But there remains the problem whether the shift in velocity is predictable enough for such an offsetting policy.

Finally, there is the problem of the advantages and disadvantages of a tight money policy, apart from its stabilization effects. Clearly these factors must be evaluated before one can make a rational decision whether to advocate a tight money policy.

But such a listing of areas of ignorance is not sufficient, for it does not give us a measure of the degree of ignorance, and hence one does not know how seriously to take it. Surely in every field of human endeavor one can, with very little assiduity compile a list of areas of ignorance. Let us therefore look at the central question: do we know enough about monetary policy for discretionary actions to be more stabilizing than destabilizing? As Friedman has shown, this requires that we guess right more than 50 per cent of the time, and the stronger the monetary policy, the greater is the proportion of times we have to guess right. Neither Professor Kareken's paper nor my own comments have really provided an answer to this question, and I feel that our ignorance about our degree of ignorance—in other words, our metaignorance —is so great that at present one cannot tell on a priori grounds whether discretionary monetary policy is stabilizing rather than destabilizing. And looking at history does not solve the problem either, for even if one accepts Friedman's evaluation of past monetary actions—something Federal Reserve officials would presumably not be willing to do—it may be that monetary policy is more effective now than it was in the past. In the close to fifty years which have passed since the passage of the Federal Reserve Act, we may have learned something, even if only what not to do again.

Basically the argument about the efficacy of discretionary monetary policy can, at present, be won only by throwing the burden of the proof on the other side, since neither side is really able to prove its case. Unfortunately, there seems to be no easy way of deciding where the burden of the proof ought to rest. It is therefore rather disturbing to note that our money and banking

textbooks as well as our principles texts generally seem to accept the effectiveness of discretionary monetary policy as though it were a proven theorem.

But, like Professor Kareken, I am less pessimistic about the future than about the present. Even if we do not have the answers, I feel that we are at least asking the right questions and that the current flood of empirical work on monetary policy will soon provide answers. Perhaps the American Economic Association should schedule another panel on this topic in 1970.

RICHARD A. MUSGRAVE: Dr. Brunner proposes that monetary analysis be divided into two steps. Step I is to examine the effects of monetary policy variables (changes in the reserve ratio and in Federal Reserve credit) on the money supply in the hands of the nonbank public; and step II is to trace the effects of changes in this money supply on such target variables as income and prices.

Step I is to be considered a problem in money supply theory, to be solved in terms of general equilibrium analysis of portfolio behavior in the money and capital markets. The model involves a demand function for debt instruments by the banking sector, and a supply function of debt instruments by the nonbank sector. Income appears as a variable in the supply function only and is assigned a quite minor role.

Step II is to be looked at as a problem in money demand theory, the demand for money by the nonbank sector being determined primarily in relation to income. Portfolio considerations may enter but are assigned a quite minor role.

In short, the theory divides into two subsets. Set 1, which deals with the supply of money is dominated by portfolio considerations, while set 2, which deals with the demand for money, is dominated by income considerations. This dichotomy has the appeal of neatness and Dr. Brunner shows it to be compatible with statistical finding. However, the same data support more than one hypothesis, and the underlying theoretical framework leaves me most uncomfortable.

I begin with step II, or the desire of the nonbank public to hold money. Brunner relates this desire to income, but the link is not the traditional one of transaction demand. Rather he adopts Friedman's explanation whereby velocity moves inversely to income in the long run, due to money being in the nature of a luxury asset; and velocity moves with income in the short run, due to acquisition of money being in the nature of consumer purchases, which, in turn, are related to lifetime rather than to current income.

I fail to be convinced by either hypothesis. From the long-run point of view, the rising money-to-income ratio may be explained as well by structural changes in the economy (e.g., increasing complexity of the financial system) which are independent of wealth, but raise the importance of liquidity and hence the usefulness of money as an asset. From the short-run point of view, I am even more skeptical. As Friedman himself points out, his analysis of consumption function, no less than the ratchet explanation, points to a rising asset-to-income ratio in the upswing, and this suggests that velocity should fall. Since in fact velocity does rise, one might be led to discard an explana-

tion in terms of consumer behavior and look to shifting asset preferences, along the lines of liquidity-preference analysis. And this the more so, as less than one-third of demand deposits are held by consumers and over two-thirds by business units of various kinds. The hypothesis that the demand for money is akin to outlays on consumer durables strikes me as most unlikely, de-emphasizing as it does the importance of portfolio preference at the very time when it should be greatest; i.e., in the short-run context of cyclical instability. No doubt, a satisfactory fit can be obtained to suit the hypothesis, but other explanations may fit as well or better. As far as liquidity preference is concerned, I am not at all disturbed that no simple fit can be obtained for a stable function. If there *is* something wrong with the Keynesian formulation of liquidity preference (apart from the disregard of change in price level), it is precisely that the theory was conceived of as part of an equilibrium system whereas, in fact, liquidity preference is inherently a phenomena of disequilibrium and change.

Turning now to step I of the Brunner dichotomy, I find it difficult to see how the equilibrium in the asset market—Brunner's supply of money—can be determined without linkage to income and prices. Only in the simplest world of 100 per cent reserves can there be a sharp distinction between changes in money supply in the hands of the public and resulting effects on income and prices. In such a world, the change in money supply is converted into a policy parameter and drops out as a problem of analysis. Only step II of the Brunner dichotomy remains, involving elements of both demand and supply analysis.

In the more complex world of fractional reserves, adjustments in the banking sector must be looked upon as an inherent part of the total adjustment in the economy. How else can we account for the effects of expectations on portfolio behavior in the bank as well as the nonbank sectors? Dr. Brunner warns that monetary authorities should not look upon changes in money supply and not on changes in reserves as a key factor in policy adjustments. I see his point but doubt its importance, because changes in the public's currency to deposit preference may be readily predicted and accounted for. However, I will go him one better and warn that changes in money supply should not be looked upon as the key factor either. What matters are the resulting changes in the level of income and prices, and these may come about with or without changes in money supply. In other words, changes in reserve ratio and/or reserve base are the policy parameters, the level of expenditures (or income and prices) are the target variables, and the money supply held by the nonbank public is an endogenous variable, determined by the interaction of the system. Brunner's division of the problem into two subsets which are determined by essentially different forces does not seem realistic to me; and if the same variables—such as changes in income, interest, and prices—enter into both subsets, the division becomes artificial and general analysis is to be preferred.

Finally, a word regarding the chain of substitution by which portfolio adjustments give rise to changes in the level of capital formation. While I am an enthusiastic adherent of the notion that monetary theory should be broadened into a general theory of portfolio preference and capital markets, I am

anxious not to let this become an analysis of stock equilibrium only. As such it is too easy a problem to complicate and hence too tempting a task for monetary theorists. As portfolio adjustments occur and the prices of existing assets change, just what is the process by which these changes may give rise to changes in the level of capital formation? In part, this returns us to the conventional issue of how capital formation is affected by changes in the rate of interest; but there is also added the effect of changes in the prices of equity assets and their repercussion (via changes in the profitability of financial investment) on the demand for real assets and thence on the level of real capital formation. As I see it, this is the dimension that must be added to traditional liquidity-preference analysis, and it is here that we are most in need of research.

MACROECONOMIC THEORIES OF INCOME DISTRIBUTION

REAL VERSUS PRICE THEORIES OF DISTRIBUTION

By SIDNEY WEINTRAUB
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To begin with what may be a trifling matter in itself, I originally titled this as a contrast between "real versus monetary" distributive theories. It appears now that this would have been a false distinction, suggesting that somehow it was a contrast between a barter and monetary economy, in its distributive aspects, that I sought, so that a "neutral" money system—to use what may be an old-fashioned and forbidden thought—could dissolve the problem. I think, however, that the issues go instead to a "roundabout" real income system, primarily a wage system in which prices evolve. The latter is the economic system in which we live and which it is our task to diagnose.

The "Own-Product" Formulation

Judging from what we still seem to teach, current distribution theory remains largely an "own-product" formulation which reveals our heritage from Jevons, Clark, and Marshall (not to neglect Ricardo on rent), with some Knight, Pigou, and Chamberlin variations closer to our own day. What are the difficulties with the textbook theories of marginal productivity analysis, where a price *numeraire* is merely used for didactic convenience and is not intended to distinguish the argument from the real results of an underlying barter phenomenon?

As a most obvious failure, this theory has never succeeded in explaining a profit share as a normal income even though it would seem that in a capitalist economy it should be easy to trace the sources of profits and account for their magnitude and changes in them. Even the Knightian theory fails here, for to explain the continuity of profits it relies on continuous change and incomplete adaptability to the shocks. But then, if the economy contains long periods of reasonable stability, the profit share should vanish. However, if one argues that the genesis of profits lies elsewhere, then this portrayal of the "annihilation" of profits in circumstances where changes were small, where cyclical fluctuations were at a minimum, and structural shifts were nominal—and granting the mobility postulates—is inapplicable; the profit sums would persist even through periods of time presumably quite ample for the equilibrating mechanism to perform its ameliorating duties.

Productivity theories are scarcely more successful in explaining interest levels. Nowadays, we do proceed by way of liquidity preference or loanable funds ideas, and then extend ourselves to see how we can make these compatible with marginal productivity concepts. Our modern interest theory *is* a price theory, not a *real* theory such as was held by Böhm-Bawerk or Wicksell. This is the one income category which has broken out of the traditional productivity strongholds—or fetters, according to one's opinions.

In the theory of rent and quasi-rent there is more to be said for the traditional theory which provides insight into the value problem for instruments of production. Yet even here the illumination extends to a theory of valuation, of imputation, of the economic significance of productive agents—but not necessarily for a theory of distribution. For to perform accurately the economic computation envisaged by the theory, it must be calculated either *ex post* where all the facts are in or *ex ante* where the valuing subjects are acting under full certainty. As neither set of circumstances is realized, the connection between the economic theory we teach and the actual distributive events of rental payments for the use of the self-same instruments is often remarkably remote and incomplete. What it has to do with actual income payments is a small puzzle. If we insist we all know better than to accept the simple theory, we should in our writings at least indicate the qualifications that we regard as minimal and indispensable to bring the ideas into greater touch with events.

In wage theory the productivity analysis may fare better. Still, here too there are some major shortcomings in the customary telling. There is first the belief, erroneous except in special ($=$trivial) cases, that the theory of "derived demand" applies. Surely we cannot seriously argue that product demand curves, expressed in money terms, hold firm regardless of whether the money wage is a penny, a dollar, or a thousand dollars per hour. Yet this is what we seem so often to be saying. When the argument is cast in real terms, there is the parallel question of whether there is not some interdependence between the real wage and the demand for labor—the theory of derived demand once again. I think there is interdependence, even a fairly strong tie at times; explanations which neglect this theoretical (and empirical) issue are thus, at a minimum, incomplete.

While other aspects of distributive theory could be cited, this setting is sufficient to indicate where the theory must be brought into closer contact with the more important distributive events.[1] Needless to say, in what follows the account must necessarily be quite sketchy.[2]

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[2] Ironically, the "own-product" formulation would fit the facts reasonably well in those cases where it is rather superfluous; that is, in those simple situations of an owner-

The Distributive Categories

It can be argued that the main distributive categories are those of wage incomes, of fixed incomes, and of profit incomes, defining the latter broadly, and in gross terms temporarily, to include all the residual sums after the allotment of the firm's income to payments for personal services—wages and salaries—and other payments, usually under contract and according to a fixed schedule, for the use of property or money capital (or its equivalent). These are the only categories that we are entitled to infer from our price theory and extend to the distributive sphere. In price theory we are accustomed to stress: (1) variable charges—mainly wage costs; (2) fixed costs—the type that gives rise to fixed incomes; and (3) profits—the residual which is maximized in the typical analysis.

On this basis, the unity of the two spheres—of price theory and distributive theory—can be maintained, with the cost categories of the one having income counterparts in the other.

As in the past—and on this there is no dispute other than in approach and nuance—the theory of wages remains in the middle of the theory of distribution. The theory should be a theory of money wages, with the real wage implications also made explicit, for this is what our students— and profession in the important matters—will talk about and be interested in. It is our task to illuminate this subject rather than leave students with the need to make a (bad?) translation from real to money wages when once they leave our classroom.

Leaving the wage theory and the wage bill which involves simultaneously the theory of employment as inextricably bound to it, distributive theory must concern itself with the fixed income shares, the *rentier* incomes of one type or another resulting from the contractual agreements between firms and resource owners, between those who require

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proprietor of land and equipment, using his personal money-capital while hiring labor under contract. Marginal productivity analysis could be suitable to explain the hire-decisions for his working force while rent theory could help in the imputation process, in showing the significance of land to his operation, and quasi-rents for the importance of his equipment. Familiar difficulties of isolating separate products from a joint contribution no doubt abound; but these we can put aside. Likewise, the theory of opportunity cost could help distinguish an interest return, a market wage, and any differential earnings, to be termed either a "wage" to extraordinary ability, or a "rent" for the self-same talents, or a "profit" for the identical enterpreneurial genius. But is productivity theory really important in this case—except for the labor hire decision? For the use of all other "owned-factors" and their incomes are explicable in terms of the theory of opportunity costs, while the separate characterization of the different types of income becomes rather pointless when they wind up in precisely the same coffers. This case hardly requires a share characterization much as it needs the principle of opportunity costs for the property and the labor income. But all this is to dwell at unnecessary length on a minor case and provide profound analysis for a simple situation which forms only a small part of our economic life.

¹ A fuller statement appears in my volume on *An Approach to the Theory of Income Distribution* (1958).

an *ex ante* agreement before committing services of factors owned by them and those, acting usually for an impersonal corporate entity, who are willing to commit a payments flow in anticipation of receipts in the belief that a positive residue of "profits" will redound to ownership—even though the mode in which the residual is split may be the outcome of intrafirm conflicts, power positions, and compromise.

In explaining the arbitrament of *rentier* incomes, ample scope remains for the traditional theory of rent, but as an *ex ante* estimate of the imputed significance of desirable land or buildings or equipment in existence and available for hire. Disparities in estimates of productiveness lead to the rental agreements. Similarly, the analysis retains the concept of the marginal efficiency of capital and the marginal productivity of investment as conditioning decisions to borrow on long-term in order to install equipment. Note, however, that while these ideas govern the investment decision, it is the long-term borrowing commitment which remains as the distributive force in ensuring a contractual stream of income payments.

In this part of the distribution study, the theory of interest plays only a fleeting role. Its connection is with employment and with the income level, with conditioning borrowing decisions which affect distributive patterns over time as the borrowing agreement is enforced, and not as a determinant of the income division at any moment or interval of time. Today's interest rate and today's borrowing—or this year's rate—affects the income flow and the size of the *rentier* accounts over the future as interest payments are made in subsequent time. Largely, interest payments are not a current distributive category explicable in productivity terms for a current contribution by a productive agent.[3]

Also under the heading of contractual payments would come a discussion of fixed government charges, for corporate property taxes, licenses, or the like. Thus the class of *rentier* income is broadened beyond usual conceptions, with the property tax-take recognized as a tenuous sort of fixed income, considering the possibilities of altering the tax base on the part of the government as the *rentier*, without any prior acquiescence of the distributive agent, the firm.

After distinguishing variable and fixed income varieties that are commonplace in the corporate structure, there remains that ill-assorted compound of residuals that I have denoted as profits—gross profits really. The big question here is the treatment of depreciation, whether it

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[3] As one minor qualification to this, insofar as borrowing takes places at short term in order to provide transaction balances or to carry inventory or the like, interest payments do represent a current distributive form, linked to the immediate production volume (and price level) and are thus a variable income on a parity with wages. Interest payments made under this heading are likely to be smaller than those attributable to long-term contract where the current payments bear little relationship to the current level of activity and are thus of a *rentier* variety.

is not also best regarded as a fixed rather than a residual income—of course, the former is the usual accounting treatment. But if we wish to separate payment of firms to owners and nonowners, where the latter are also dichotomized as wage and non-wage factors, then depreciation belongs with the gross profit category. But this is a minor matter of pigeonholing among all the residual sums whose resolution is not specially significant on a general view. Among the other income categories with claims on this residual are dividend recipients, bonus participants, government by way of income tax, internal finance through withholding, etc.

Realism and Importance

The virtue of this classification (or one close to it) is not only its realistic content but, more important, the significance of its categories. For unless we are resolved to discuss actual income payments, or the determinants of shares to major income type or class groupings, it is hard to understand what the theory is all about. Is it to refer to categories of productive agents for which we find no payment counterparts? Unless there is place for the distinction between fixed and variable incomes, for example, it is difficult to comprehend any serious concern with inflation. Consequences of the latter weigh heavily in the field of income division. But these excesses which stem from price-level instability are lost in the traditional discussions unless dragged in quite artificially in an account of quasi-rent. Unless distinctions of this sort are made, we scarcely can talk to any purpose on actual corporate profits or explain how under particular tax laws they can be legally augmented by accounting procedures which tolerate a variety of depreciation practices.[4] These are legitimate inquiries for a theory of distribution; they arise in a price economy and are difficult to envisage in a "real" system.

How far removed from life the older categories are—and their negative impact on empirical investigations—can be sensed from the fact that there seem to be no useful estimates of the magnitude of fixed or *rentier* income in our society. We have estimates of wages, or rent, and of interest. Yet the rental sums are merely a compilation of an ill-assorted array of payments, some on short-term contract, others on long. The similarity in language to economic rent suggests that the income aggregate is that of the older theory. The fact is that the two are quite far apart. The same is true of the sum of net interest payments, some of which are payments for current loan finance—an appropriate variable cost category and distributive payment linked to current factor

use—while the rest consists of a contractual *rentier* sum bearing little relationship to current interest rates or economic activity in general. An empirical study which isolated the income categories in the way suggested is badly needed. It would also lead us to estimate the magnitude of "forced savings" in our system during inflationary epochs and restore this concept to its rightful place in our thinking.

Rentier Versus Profit Incomes

This three-part grouping is preferable to the older and often revived duel between wage and capitalist income. The latter is apt to overlook the fact that not all "capitalists"—non-wage earners really—fare in the same way in an income advance or recession. Important differences are obscured when *rentiers* are lumped with profit recipients: their fortunes are not identical; their interests in inflation, in higher levels of activity, in the exercise of monopoly power, etc., do not coincide. Of course, many in the one also appear in the other, so that for this overlapping group any separation is superfluous. However, where the groups are distinct, then real income shifts and conflicts among them can occur in a variety of circumstances, such as with variable output levels or variable price levels. It is the task of theory to separate these strands and ascertain the distributive incidence of diverse economic phenomena rather than to hide it.[5] Unless propensities to consume of the two groupings are roughly equivalent, further repercussions on the productive process can ensue from distributive shifts. It is regrettable that empirical studies have not given us a good clue to the number and average economic position of *rentiers* as against profit recipients, and as against wage earners. And yet we are so willing to discuss the effects of inflation—and economic policy generally—upon distribution—despite the empirical darkness in this area. Probably the failure of theory to point up the question accounts for the paucity of empirical findings.

The Theory of Relative Shares

Once the old categories are abandoned in favor of a new listing, the theory of relative shares comes to the fore rather quickly. The older theory was primarily interpreted as a theory of factor prices—and thus as an extension of value (or price) theory. Intermittently there was some fascination with the theory of relative shares, of how class incomes fared relatively to one another and the total. To mind come Ricardo's landlords and capitalists, Marx's immiseration of labor, and Kalecki's prewar study indicating a great constancy on the part of wage incomes in the income total.

[5] Thus I think the use of a Cobb-Douglas function, which distinguishes wage and profit income, to be unacceptable for distributive analysis; for it, too, submerges differences among recipients of the "capital" share into simple likenesses.

More recently, however, Professor Kravis has used national income estimates to show the increase in the wage share since the early years of this century.[6] Professor Solow, too, has been skeptical of the proclaimed wage share stability, also using national income ideas.[7]

If one works with Business Gross Product data of the Department of Commerce, near-stability in the wage and salary share can be revealed.[8] It appears, too, that the same near-stability can be discerned in reaching back to the early years of this century. Every indication is that this firmness has largely persisted despite the often cataclysmic changes in our economy as judged by any test.[9] Assuming this is so, that most intriguing question arises; namely, why the stability?

On this point economists may have been derelict in giving so little attention to the analytic aspects of the problem. The stability was likened to a "miracle" by Keynes while Mrs. Robinson remarked that the failure to grapple with it was a "reproach to theoretical economics." The number of serious efforts to deal with the phenomenon can be tabulated on those primitive counting devices given to all of us—our fingers.[10] Yet so much remains to be done, with many relevant questions.

First, is the stability merely a statistical illusion resulting from the choice of data? Should national income statistics be used?

Undoubtedly we welcome those studies of wage income in national income, which contain the income stream, not only of our business sector in the mixed economy, but also the government sector, some household accounts, some charitable institutions, and the international income items, etc. While it is useful to trace through this total income stream, it still remains that if we are interested in learning what the business sector does, how the capitalist sector of the economy operates —and this was the Ricardian-Marxian problem—we had better confine ourselves to data which belong to the business part of the economy. This is all the more so if we wish to compute ratios of capital per head and capital/output ratios. Most of these have meaning solely in the business sector.

One further remark on the choice of an income concept. As we go back some fifty or even thirty years, to the earlier 1900's, say, and make national income comparisons, what we are doing is comparing data of an essentially straight business economy at that time to one of business and much more government now. A "purer" series of income

[6] Irving Kravis, "Relative Income Shares in Fact and Theory," *A.E.R.*, Dec., 1959.

[7] Robert Solow, "A Skeptical Note on the Constancy of Relative Shares," *ibid.*, Sept., 1958.

[8] See my *A General Theory of the Price Level*, etc. (Chilton Book Co., 1959).

[9] A dissertation in process by a student of mine, Arthur Grant, "The Wage Share in the Business Product Since 1900."

[10] See Paul Davidson, *Theories of Aggregate Income Distribution* (Rutgers Univ. Press, 1960).

events is embraced by the figures for the business sector rather than for the whole economy.[11]

Next, there is the question of using gross versus net data, or net versus gross business income. My preference is for the latter, for several reasons: (1) this is the nearest to the sales revenue concept of price theory and so permits the readiest application of the developed framework of ideas from the latter sphere; (2) depreciation allowances and tax sums—which largely distinguish the gross from the net estimates—are distributive allocations out of proceeds; (3) tax laws render it profitable to swell depreciation figures in order to minimize profits and the tax bite; on this basis depreciation estimates are probably overstated and profits understated; (4) it has always been an easy matter to question the strict economic validity of the statistical estimates of depreciation; (5) depreciation (plus tax) deductions have grown relatively in recent years. To use net figures in historical series thus involves making disproportionate allowances for capital claimants on the business revenue dollar. In the early years this claim was minor while now it is so much more important. For these reasons, a case for the gross figures can be made. Nonetheless, even if this view is rejected, the fundamental question must still remain to plague any critics; namely, why has the share of labor, as represented by wages and salaries, remained nearly constant in the gross business product accounts? We cannot answer this by evading it, by arguing that less constancy is shown when another income concept is substituted.

Of course it would be nice if wages were somehow separated from salaries in these estimates. Undoubtedly a host of definitional problems, of ambiguities, and even absurdities might arise in any attempt to break down the present aggregates, especially in smaller enterprises. I have seen one study which purports to show strong rigidity in the income share of "production workers." It is hard to judge whether other studies will confirm this.

If a breakdown indicates an important shift from wages to salaries, then we have an additional phenomenon that warrants study. Would this be merely a consequence of the search for status and thus an upgrading of personnel? Does it portend that the personal labor income sector is becoming more rigid and that wage—now salary—costs are more like *rentier* income? There are some interesting implications for pricing in this. Historically, it may also reveal some unnoticed phenomena in the story of development.[12]

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[11] Thus a strong case can be made for avoiding the use of complete national income data when we want to refer to concepts that are strictly relevant only to market economy—concepts such as the price level, the market income division, labor productivity in industrial production, the influence of monopoly, technological unemployment, etc.

[12] It is rather distressing to find so much discomfort over the various evidences of cer-

A Capital Output and Wage Share Truism

While indicating the problems, this is not the place to develop at any length the theory of relative shares. Earlier I did point out that the elasticity of productivity or the relation of marginal to average product was crucial in this context, along with the degree of monopoly power.[13] Also, that changes in the wage share depended on the money wage and gross proceeds—involving really a "law of large numbers," or a small magnitude compared to a large—and the elasticity of aggregate supply.[14] One other interesting relation should be pointed out inasmuch as it ties the analysis to much of contemporary empirical work. From the price-level truism, $P = k\ w/A$, we can write:[15]

$$\frac{1}{k} = \frac{w}{P}\left(\frac{1}{A}\right). \tag{1}$$

While (1) indicates that the wage share $(1/k)$ depends on the real wage (w/P) and the laws of return embodied in A, this can be carried somewhat further, to a more revealing relationship. For $1/k$, which is the wage share, we can write W_y. Multiplying through by the value of the stock of capital equipment, K, and writing w_r for the real wage (w/P), we have

$$W_y = w_r \frac{N}{Y} \frac{K}{K} \tag{2}$$

$$W_y = w_r \frac{K_y}{K_n}. \tag{3}$$

Equation (3) is the interesting one. It reveals that the wage share is equal to the real wage, multiplied by the capital-output ratio (K_y), divided by the magnitude of capital per head, K_n.

Some inferences are obvious. If K_y is constant, as it often is, the wage share depends on how real wages move relative to capital per head.

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tain important constancies in economics, in saving ratios, capital-output ratios, productivity developments, and not least, the labor share. Why do we fear these? Why do we refuse to acknowledge them as empirical facts and utilize them in other aspects of our work even as we proceed to understand them and explain them? I have even encountered the twisted argument that the recognition of a (nebulous) functional relationship imparts more insight than the acknowledgment of an empirical (near) constant. Is this anything short of arguing that a horizontal locus in a plane diagram, almost regardless of the associated variable, is a weaker analytic form than a curved locus whose shape we can fix only vaguely, whose location is in doubt, and whose stability is in question? Does work with such fuzzy "functions" advance our stature as a science in the same way as empirical recognition and analytical utilization of (near) constants in other functional connections?

[13] *An Approach*, pp. 51, 66.

[14] *Ibid.*, p. 49. To the elasticity can be added a term for the degree of monopoly power.

[15] See my *A General Theory of the Price Level*, p. 9. *P* refers to the price level, *w* to the average money wage, *A* to the average productivity per employee, and *k* to the markup factor or reciprocal of the wage bill to gross income proceeds.

Only where the latter variables move proportionately would the wage share hold constant. Also, it is not surprising that in an economy in which K_y is largely constant that W_y will be highly stable. The two are immutably bound together.

More can be done with (3), empirically and analytically. The functional interdependence and obvious connections among the separate variables deserve attention not only in connection with the theory of distribution but in the theory of growth.[16]

Non-wage Shares

The obverse of the constancy of the wage-salary share is the similar constancy of the non-wage share, the capitalist income divided in the main between *rentiers* and profit recipients.

As remarked, it is essential to distinguish at least these two incomes rather than lump them together as capitalist income. For the income fortunes of these two groupings, *rentiers* and entrepreneurs (or capitalists proper) are not wholly compatible. They do not coincide and, despite a common belief in the identity of interests on the part of the income occupants in each grouping, their well-being may conflict as often as not. Of course, the two classes overlap, probably to a large degree. The same individual is a bondholder and a stockholder of the same corporation, or a bondholder in one, a stockholder in another.

So far as the *rentier* goes, he is interested specifically in economic activity being large enough to permit contractual commitments to him to be honored. Beyond that, especially if greater activity means a higher price level, his real income is bound to suffer, to be affected adversely. In general, the *rentier* share declines in a money income advance whether brought on by a price rise or output rise at constant prices. Real income, however, only erodes as prices rise somewhat, as they almost invariably do as employment and production quicken. It is this group that is victimized by full employment and inflation and, barring a wave of bankruptcies, the tacit beneficiaries of underemployment.

The profit share, on the other hand, is the most volatile portion of the total, fluctuating with each tide in gross business product, whether brought on by price movements or output movements. It is the entre-

[16] Without using this truism (3), and in the context of the theory of growth, Mrs. Robinson has emphasized the same variables in arguing that: "The important thing is to keep a firm grasp on the distinction between value of capital per unit of output and . . . per man employed. In technically progressive economies we expect to find capital per unit of output more or less constant over the long run while capital per man employed is steadily rising." See her article, "Some Problems of Definition and Measurement of Capital," *Oxford Econ. Papers*, June, 1959, p. 153. Professor R. F. Kahn has said much the same thing in his article on "Exercises in the Analysis of Growth," *ibid.*, p. 147. Needless to say, modern empirical work is tending to utilize these ideas and it can be done more systematically and more perceptively if the truistic nature of the arguments is before us.

preneurial group that has the most vital stake in full employment, and which also benefits most from inflation.[17]

More undoubtedly could be done in both associating and decomposing the profit and *rentier* categories. I would press the one further, and major, conclusion on the different stake of the three groups in full employment, especially if this entails (as I think it usually will) somewhat higher prices. Profit incomes alone move up as full employment is reached on an uphill climb. It is even wrong to argue that labor benefits from the erasing of unemployment; actually only the "null-income set" —the formerly unemployed—benefits from fuller employment. Those already employed will often find a fall in their real income as a penalty of full employment as prices go up. On this basis, there is reason to expect that public policy for full employment will encounter more resistance the greater its success in absorbing unemployment. For as the null-income set contracts—as the unemployed pool becomes smaller— the greater the opposition to further public measures to improve the aggregate employment position. Antigovernment, anti-inflation, antisocialism, and all similar arguments make most headway as success toward full employment is more nearly achieved.

Real Versus Price Theories of Distribution

I began with a brief critique of real theories of distribution and then went on to distinguish the income categories and the paramount distributive problems in a price economy. It may be well to point up some further matters which go undiscussed in older views and become of central importance in the price economy.

The role of contracts, for example, as a phenomenon responsible for profits has been mentioned. More might be done on the relation of contracts to the enterprise system, contracts for a viable competitive system, and contracts and the public interest. Here is a veritable field day for the institutionalist and for the border studies of law and economics— currently unfashionable in our profession.

Again, it is only in a price economy that a discussion of inflation and its distributive aspects becomes meaningful. Now price-level inflation (and deflation) seems always to be with us and yet, in the real-economy

[17] That *rentier* and entrepreneurial attitudes are not fully compatible is manifest in numerous political pressures and actions of these two capitalistic groups whose economic interests are too casually regarded as identical. Intellectual confusion on this point exists on the part of the businessman through the greater articulateness of organs of *rentier* opinion which mold ideas and lead the former to believe that the views espoused are serving a mutual end. Still, condemnations of "Wall Street," differences on easy and tight money, on tariff policy, government expenditure for regional or industrial programs are commonplace. Only in the quite universal rejection of taxes would a full community of ideas appear! Here, too, wage earners could join the self-same ranks of an anti-tax party; differences in magnitudes probably explain most of the differences in the volume and vociferousness of the protests.

analysis, the distributive phenomena get lost in the shuffle so that this vital matter only emerges in other chapters in economics.

Further, aside from leading to a theory of money wages—and we should have one or attempt to achieve one, for otherwise we are deceiving our students and audience when we offer pontifical pronouncements on commonplace wage phenomena—the approach recommended leads to an appropriate theory of interest rates, with certain distributive complications. For example, if the wage level largely governs the price level, the money wage level also, through the transactions demand for cash balances, affects the various rates of interest. Thus not only is the level of activity affected, with certain distributive consequences when employment and wages weaken or strengthen, but also long-term contracts and the *rentier* pattern of claims over time are influenced. Furthermore, there are interest payments on short-term finance: higher or lower bank rates thus affect the distributive scheme with an impact largely upon profits; this must be examined at constant and varying output levels. Short-term interest rate changes thus affect interest payments as a variable income similar to wages. Justification for neglecting this category (as distinct from long-term contractual interest payments which are a component of the *rentier* payments) must be premised on their relative unimportance.

Clearly, interest payments for transactions balances or the medium of exchange function of money fail to make any appearance in the real economy. This, too, would be a reason, maybe only a small one, for confining the distributive analysis to a price economy.

Finally, if I am correct in arguing that there are important distributive shifts as the price level varies with output constant and with output varying, then an important section of the theory of income distribution consists of an analysis of these shifts on the magnitude of aggregate demand.[18] These are important matters not readily embraced by a theory that discards the "veil of money" and adverts to barter-type, real-term analysis. While a real analysis in a price framework may be trickier and more involved, some of the difficulty may stem merely from its unfamiliarity.

[18] See my *Approach*, Chap. 5.

EFFECTS UPON THE DISTRIBUTION OF INCOME OF A TIGHT MONEY POLICY*

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The Nature of the Analysis

This analysis is designed to estimate the effects upon the distribution of personal income of monetary actions to stabilize the general level of prices for goods and services. In particular, we attempted to estimate how the "average" income receiver in a given personal income size class would fare—with respect to his real income—if there were an increase in the demand for goods and services and (1) no offsetting monetary or fiscal action were taken so that the price level rose, (2) monetary action were taken to keep the price level at its initial value, or (3) no monetary action were taken but (*a*) some tax rates were increased to keep the price level from rising and (*b*) federal expenditures were reduced without any offsetting change in tax rates. It is assumed that anti-inflationary monetary action would result in increased interest rates, whereas anti-inflationary fiscal action could keep both the price level and the interest rate from increasing.

Our estimates are necessarily relatively crude ones. The analytical framework ignores certain impacts upon the economy that will influence the distribution of income. And, of greater importance, we have been able to obtain only crude descriptions regarding many of the important "facts"; e.g., who holds various kinds of assets and liabilities, the amounts borrowed and loaned by various economic units, how interest rates affect purchases, the incidence of various kinds of taxes and the wage and profit structures in various industries.

From among the many different kinds of stabilizing fiscal actions that could be taken, only three different changes in the tax pattern and one change in federal spending have been chosen for consideration in this analysis.

The estimates that are to be presented are for the average income receiver in each income class. Few, if any, income receivers would be affected in exactly the same way as one of these average income receivers and a sizable fraction may experience changes in the direction opposite to that in which the average has moved. Although we have not described quantitatively the variation in income effects within the various income classes, available data indicate that this variation is relatively large, due

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* This is a brief summary statement of the results of a study commissioned and financed by the Commission on Money and Credit. Much of the computation was carried out by Mr. Mikoto Usui, Massachusetts Institute of Technology.

to differences in preferences, age, family status, and the many other characteristics influencing the economic behavior of income-receiving units with a given current income.

Increased interest rates mean increased borrowing costs and increased alternative returns from saving rather than spending. These changes will influence decisions to purchase durables and to save, both directly and via their effects upon the capitalized value of income. Comparisons at given equilibrium values emphasize substitution between durables and nondurables, with no change in total resources employed. In the analysis of employment effects, however, we assume a net reduction in employment, the substitution taking place between present purchasing and saving.

Restrictive fiscal policy may operate by reducing government expenditures or by increasing taxes or, of course, by some combination of these two possibilities. A smaller total of government spending, accompanied by no change in taxation (or, at least, by a smaller reduction in the tax bill), will reduce the flow of incomes throughout the economy, private as well as public. Correspondingly, increased taxes at a given level of income mean lower disposable income and hence reduced spending for goods and services if expenditure depends upon either income or wealth.

By taxing more (or expending less) and purchasing debt, the government could counter an inflationary disturbance and keep the interest rate at a lower level than if it reduced the money supply. The budget action will reduce total spending at given interest rates; the retirement of debt will reduce the interest rate and therefore increase private spending.

The redistribution of income resulting from inflation is a transfer from people with assets whose nominal values increase less rapidly than the price level to people with assets whose nominal values increase faster than the price level. In particular, creditors lose and debtors gain from inflation. Some wages and commodity prices may rise proportionately more than others because of institutional factors. However, such possibilities are not analyzed in this study.

Increased interest rates will result in a revaluation of streams of income from assets yielding fixed flows and, therefore, in the market prices of existing assets. Relative market values may change, since assets have different durabilities or maturities, but unless there are actual sales, there is no necessary change in income flows as a result of the increased rates. New contracts, however, will be made at higher interest rates, thus resulting in a shift of income from interest payers to interest receivers. Among the interest payers should be included taxpayers in their capacities as servicers of certain governmental debt.

If the aggregate level of economic activity were not affected by the interest rate change, this effect would be the only redistributive one—except insofar as the demands for various goods and services are not affected proportionately. In the event that they are not and that wages and profit structures in different industries are not the same, there will be additional alterations in the income distribution. A net decline in activity levels is a special case of this change in proportions, in which activity in certain sectors declines without compensating increases elsewhere: there is a net loss to the economy.

An inflationary disturbance that is countered by reduced government expenditure or increased taxes and purchases of debt to keep the interest rate at its initial level also results in a partially compensating transfer of income from taxpayers to interest payers. Again, there may be structural changes—including a reduced over-all level of economic activity—that will affect the pattern of income.

In order to estimate quantitatively the effects of the various possibilities, we shall first assume that there has been a once-and-for-all autonomous increase in the demands for goods and services sufficient to raise the price level by some arbitrarily given amount—in the absence of any monetary or fiscal action to counter this demand increase. The redistributive effects of this possibility will be estimated according to the estimated distribution of liquid assets.

We are making the simplest assumption about the shape of the autonomous demand increase; namely, that the increase is proportionate to the 1957 composition of goods and services. Another assumption would not make any difference in the estimation of equilibrium values following from restrictive fiscal or tight money policies. However, some other distribution of the demand increase might result in different structural effects.

We shall assume, second, that whatever monetary action might be taken to counter the inflationary disturbance will exert its direct effect through increased interest rates. The increased interest charges will make expenditures upon certain classes of goods lower than otherwise would be the case, even though money income were unchanged. However, reductions in expenditures for some classes of goods will subsequently cause lower expenditures on other classes that are not sensitive to interest rate changes, because of the general reduction in income.

Few direct estimates of the interest elasticities of expenditures for various classes of goods are available. But we can make use of other knowledge about expenditure behavior and estimate these interest elasticities indirectly. Increased rates of interest increase the total payments made for an asset with a given purchase price when payment is deferred.

Furthermore, increased payments due to a higher cash price cannot be distinguished from increased payments due to higher carrying charges (because of higher interest rates). Consequently, we assume that the elasticity of demand for an asset with respect to its total cost is the same as the elasticity of demand with respect to price. If we can determine how changes in interest rates affect total costs, then we can estimate the elasticity of demand with respect to interest rates by multiplying the price elasticity by the elasticity of cost with respect to interest rates. This product is the interest elasticity of demand, if one further condition is fulfilled; namely, that interest rates affect the cost of only this good— say, houses—relative to the costs (prices) of other goods. The estimated interest elasticities are presented in Table 1.

The value assumed for the multiplier is not crucial for this analysis, since it is only a determinant of the magnitude of the change in the rate of interest or the tax change required to obtain a given equilibrium result. The absolute amounts of income transfers will be dependent upon the size of the multiplier, but whether a person gains or loses from a particular policy is not dependent upon the multiplier. We assume a multiplier of 1.4. This is consistent with a marginal propensity to spend with respect to disposable income of .70 and a net marginal leakage rate of 0.6.

The Estimates

To estimate the effects of an increase in the general level of prices upon the distribution of income, we assume that real wages and real profits are invariant with respect to the changes in prices. Real government purchases of goods and services also are assumed to remain unchanged. The real tax burden, however, is reduced, assuming that tax collections are employed to meet interest payments on government debt. Thus, the change in the distribution of income as a result of inflation will be dependent upon the amount of the increase in the price level and the distribution of federal tax collections and of income-earning assets whose nominal earnings are fixed.

Assets earning no income, namely, cash and demand deposits, as well as assets earning fixed monetary incomes will depreciate in value as a result of a rise in prices. If we assume that the marginal utility of money is the same as that of the income-earning assets, a measure of the change in the value of assets is a better approximation to the change in welfare than is a change in money income. Consequently, the effect of inflation is described according to the manner in which inflation affects the real values of asset holdings. We have assumed that there are no structural effects in the example of unopposed inflation.

TABLE 1

ESTIMATED INTEREST ELASTICITIES AND RESPONSES TO A ONE PERCENTAGE POINT INCREASE IN INTEREST RATES, 1957

Type of Asset	Per Cent Borrowed	Borrowing Rate (%)	Lending Rate (%)	Life of Instrument (years)	Life of Asset (years)	Depreciation Plus Maintenance (%)	Weighted Average Interest Rate	One Minus Marginal Tax Rate	Elasticity of Cost with Respect to the Interest Rate	Price Elasticity	Interest Elasticity	1957 Expenditures (Billions of Dollars)	Proportionate Change in Interest Rate	Change in 1957 Expenditure Due to a 1 Per Cent Change in Interest Rate (Billions of Dollars)
Construction for maintenance	0.75	6	6	5	30	3.33	6	0.7	.39	-1	-.39	15.3	.167	-1.0
Industrial	1.00	5	6	5	40	3.13	5	0.5	.29	-1	-.29	5.4	.20	-0.3
Commercial	1.00	6	6	25	40	3.13	6	0.5	.32	-1	-.32	24.4	.167	-1.3
Residential	0.80	6	6	25	60	2.08	6	0.9	.56	-1	-.56	11.7	.17	-1.1
Highways	1.00	4	6	25	25	5.00	4	1.-	.286	-⅓	-.14	5.1	.25	-0.2
Institutional	1.00	5	6	30	50	2.5	5	1.-	.5	-⅓	-.25	4.5	.20	-0.2
Producers' durables	1.00	6	6	5	15	8.33	6	0.5	.15	-1	-.15	25.0	.167	-0.6
Farm machinery	.80	10	6	4	15	8.33	7.1	0.9	.28	-1	-.28	1.1	.14	-.04
Autos	.70	12	6	2.75	10	12.5	7.4	0.9	.21	-1	-.21	13.2	.14	-.39
Durable consumers goods	.90	15	6	2.5	10	12.5	8	0.9	.22	-1	-.22	20.0	.125	-.55
Total														-5.68

A 3 per cent price increase in 1957 would have been detrimental to the average income receivers in the lower income classes and beneficial to those in the upper income classes. The estimates are shown in Table 2.

A change in interest rates will change the flows of interest payments and receipts. The manner in which these flows will be altered was estimated from data relating to the structure of assets and liabilities according to size of income. Our estimates imply that borrowing and lending patterns among the income-size classes are the same after the interest rate change as they were before. The absolute levels of borrowing and lending may be different, but the relative net asset positions are assumed to be unchanged.

A 3 per cent increase in prices—with no change in real output—in 1957 would have meant an increase in gross national product of approximately 13 billion dollars. With a multiplier of 1.4, interest rates would have had to be increased sufficiently to cut spending on account of the interest rate change alone by about 9 billion dollars—the other 4 billion reduction occurring because income would be lower than otherwise would be the case. A 1.7 per cent increase in the interest rate would have brought about approximately this reduction. The estimated increase is the amount required to keep the equilibrium level of spending from rising. No attempt is made to estimate the time period required for the new equilibrium to be achieved nor of the movement of gross national product between the initial position and the new equilibrium. If both long- and short-term Treasury rates were increased by, say, 1 per cent, it

TABLE 2

ESTIMATED EFFECT OF A 3 PER CENT INCREASE IN THE PRICE LEVEL UPON THE
AVERAGE "REAL" NET WORTH IN VARIOUS INCOME-SIZE CLASSES, 1957
(With Alternative Assumptions About the Distribution of Federal Debt*)

Income Class	Assumption I	Assumption II
	$	$
$ 1,999 or less	− 96	− 86
2,000– 2,999	− 77	− 66
3,000– 3,999	− 61	− 43
4,000– 4,999	− 64	− 41
5,000– 5,999	− 53	− 24
6,000– 7,499	− 9	+ 23
7,500– 9,999	+ 19	+ 58
10,000–14,999	+ 74	+ 98
15,000–19,999	+ 98	+ 66
20,000 and over	+667	+540

* Assumption I is that one-third of corporation income tax is distributed proportionately to consumer expenditure and two-thirds proportionately to dividend receipts. Assumption II is that two-thirds of the tax is allocated proportionately to consumer expenditure and one-third proportionately to dividend receipts.

is assumed that both rates paid by other borrowers and received by lenders would be increased by 1 per cent.

In Table 3, estimates of the increases in interest payments and receipts flowing from one income class to another are presented. If gross debt were 1 trillion dollars, gross interest payments would be increased by at least 10 billion dollars as a result of an increase of 1 per cent in interest rates. The interest that we would be able to net out would total about 4 billion dollars.

A change in interest rates also will alter the structure of the economy, since the interest elasticities of expenditure for the various classes of goods and services differ and the requirements of labor, capital equipment, and intermediate goods are not the same for each class of final goods. From input-output data for the U.S. economy, we have estimated how the activity levels of each industry would be altered if the income effects of interest rate changes on the demand for final goods were ignored. This provides an estimate of the short-run effects of the interest rate change upon the structure of the economy.

Assuming first that an index of total output were unaffected by increased interest rates, i.e., that resources moved readily from one industry to another, the differences between wage and profit structures among industries whose activity levels are most affected by interest rates and those least affected will determine the effect upon aggregate wage and profit shares due to a tight money policy. Dividing industries into two classes—those declining more than the average (and therefore declining absolutely with no change in over-all activity) and those declining less than the average (therefore increasing absolutely with no change in over-all activity), we find that the average hourly wage in 1957 for the first group was $2.68 and that for the second was $2.20. Since these differences are not statistically significant, one can say that it is unlikely that wage income would be affected by interest rate changes if total employment were unaffected.

Assume, at the other extreme, that there is no movement of resources among industries, so that a decline in activity in one sector is not offset to any extent by increases in others. The pattern of the induced reduction in demand due to interest rate increases, compared to the pattern of the autonomous demand change, is the determining factor with regard to both the decrease in output and its structure. In the special case in which all commodities have identical interest elasticities of demand and there is an across-the-board proportionate increase in demand countered by tighter money, there would be no reduction in output in any sector, even though resources were completely immobile. This would also be the outcome in the event that the sectoral increases in demand were proportional to the interest elasticities. The decline in activity in the case in

TABLE 3

ESTIMATED CHANGE IN AVERAGE (PER INCOME-RECEIVING UNIT) NET INTEREST
RECEIPTS, BY INCOME CLASSES, DUE TO A 1 PER CENT INCREASE
IN ALL INTEREST RATES, 1957

Income Class	Increase in Mortgage and Consumer Credit Payments	INCREASE IN PAYMENTS ON SHARE OF GOVERNMENT DEBT*		Increase in Payments on Share of Business Debt†
		Assumption I	Assumption II	
$ 1,999 or less....	$ 29.50	$ 9.83	$ 15.28	$ 3.84
2,000– 2,999...	30.99	18.89	25.14	4.79
3,000– 3,999...	35.11	27.61	37.59	4.93
4,000– 4,999...	37.52	34.71	47.80	6.19
5,000– 5,999...	45.71	44.68	61.11	6.87
6,000– 7,499...	53.91	57.94	76.55	8.04
7,500– 9,999...	68.90	76.47	98.26	13.04
10,000–14,999...	88.93	115.92	129.44	17.68
15,000–19,999...	102.73	202.81	184.38	28.24
20,000 and over..	140.81	926.48	871.71	108.58

Income Class	TOTAL INCREASE IN INTEREST PAID*		Increase in Receipts for Bonds, Savings, Time, and Building and Loan Deposits and Pensions and Annuities Plus Insurance	NET CHANGE IN RECEIPTS MINUS PAYMENTS*,‡	
	Assumption I	Assumption II		Assumption I	Assumption II
$ 1,999 or less....	$ 43.17	$ 48.62	$ 84.56	$ 41.39	$ 35.94
2,000– 2,999...	54.67	60.92	85.05	30.38	24.13
3,000– 3,999...	67.65	77.63	88.38	20.73	10.75
4,000– 4,999...	78.42	91.51	100.45	22.03	8.94
5,000– 5,999...	97.26	113.69	111.89	14.63	– 1.80
6,000– 7,499...	119.89	138.50	111.84	– 8.05	– 26.66
7,500– 9,999...	158.41	180.20	131.97	– 26.44	– 48.23
10,000–14,999...	222.53	236.05	158.88	– 63.65	– 77.17
15,000–19,999...	333.78	315.35	245.29	– 88.49	– 70.06
20,000 and over..	1,175.87	1,121.00	708.46	–467.41	–412.64

* See Table 2 for a description of the two assumptions.
† Allocated proportionately to consumer expenditure.
‡ Average payments will exceed average receipts for the population as a whole since allocated debt exceeds allocated assets by about 19 billion dollars.

which resources are completely immobile comes about because the autonomous increase in demand does not match the pattern of interest elasticities and, therefore, the pattern of demand reduction brought about by monetary action.

One can generalize that, if the pattern of autonomous demand increase is not matched by the induced reduction in demand due to any counter-inflationary policy and resources are immobile, there will be a net decline in activity.

Assuming that the increase in autonomous demand were proportional to the 1957 demand levels and the direct, induced declines were distributed according to the interest elasticities presented earlier, the 9 billion dollar reduction in the demand for durables would cause a

total (direct-plus-indirect) decline in gross output levels of 2.2 per cent. Since the decline in labor requirements need not be proportional to the decline in the output level in any sector and this relationship may differ from sector to sector, the induced decline in employment may be greater or smaller than the decline in activity. In fact, the amount of unemployment generated by the first-round impacts of a tight money policy, under the assumptions made above, would have been 1.6 per cent of the total man-hours in 1957. Because of the difference we noted earlier between the average wage in industries suffering greater than average declines in output as compared to those with less than average declines, the loss in compensation of employees would have been 1.9 per cent of the 1957 level or 4.4 billion dollars.

We assume that the deflationary impact of an increase in tax collections depends only upon the size of the increase and not upon its composition. The distributional effect, however, does depend upon the nature of the tax change. The effects of three changes in tax patterns are considered: equal proportionate increases in every taxpayer's federal taxes, equal proportionate increases in every taxpayer's federal personal income taxes, and an increased rate of federal corporation income taxation. The amount by which the total tax bill should be raised to counter a particular inflationary disturbance is estimated from the expenditure relation described previously.

Since some of the proceeds of increased tax collections are in effect employed to subsidize interest payers, the tax collections needed to prevent prices and interest rates from rising are offset by the reductions in interest payments. Approximately 13 billion dollars of additional taxes would need to be raised to prevent prices from rising, and this amount—collected according to the patterns described above— would be distributed among income-size groups according to the incidence patterns shown in Table 4.

Since the higher taxes, even though progressive in their direct impact, are offset by reduced interest payments, the policy of raising taxes and purchasing debt will include a partially compensating redistribution of income, to some extent at the expense of the lower income classes.

As was the case with monetary policy, an increase in the revenue bill at a given income level will have structural effects that are due to the pattern of decline in the purchases of goods and services when disposable incomes fall. These effects, assuming no compensating shifts in the flows of resources, were estimated by the use of the input-output accounts.

An increase of federal revenues of 13 billion dollars would cause a direct decline in consumer expenditures of 9 billion. With no resource mobility, the direct reduction would cause a total (direct-plus-indirect) reduction in gross output of 2.4 per cent of the 1957 level. Under this structure of sectoral declines, the amount of unemployment generated would be 1.3 per cent and the loss of compensation of employees, 1.3 per cent, or 3.2 billion dollars. If resources were perfectly mobile, the average wage rate would rise in contrast to the corresponding tight money case, since the average wage of the industries that would suffer greater-than-average declines in output due to higher taxes is $2.00 per hour, compared to $2.20 in the industries declining less than the average. (Again, these differences are not statistically significant, using Student's t.)

TABLE 4

ESTIMATED REDUCTIONS IN ANNUAL INCOME ON ACCOUNT
OF TAX INCREASES, BY INCOME-SIZE CLASS

Size of Income	All Federal Taxes Are Increased Proportionately*	Personal Income Tax Increased	Corporation Income Tax Increased*
Less than $2,000....	$ 28.83	$ 10.18	$ 44.10
$ 2,000– 2,999.....	60.24	45.78	69.88
3,000– 3,999.....	90.10	74.08	88.09
4,000– 4,999.....	115.87	106.37	96.87
5,000– 5,999.....	151.16	153.26	107.07
6,000– 7,499.....	198.67	210.76	138.21
7,500– 9,999....	264.88	285.99	153.55
10,000–14,999....	409.19	448.59	336.44
15,000–19,999....	730.11	821.37	719.97
20,000 and over....	3,487.00	3,367.14	4,816.43

* Assumes two-thirds of the tax is borne by dividend receivers and one-third by consumers

An alternative fiscal policy is a surplus brought about by reducing government expenditures without changing revenues correspondingly. Structural effects and net unemployment will occur under the conditions outlined in the previous sections, but with the pattern of decline due to a federal spending cutback replacing the pattern due to interest increases. With no resource mobility, a decline of 9 billion dollars in federal spending would cause a total (direct-plus-indirect) reduction in gross output of 1.9 per cent (1957 levels). The amount of unemployment generated would be 1.5 per cent of the 1957 total man-hours and the loss of wage and salary compensation would be 1.8 per cent or 4.3 billion dollars. The average wage in the industries declining most was $2.73 per hour, contrasted with an average of $2.25 in the indus-

tries declining relatively less. (These averages were not significantly different.)

Since the surplus generated by reduced expenditures would also be used to purchase debt, there is an interest payments subsidy in this case, as there was with the tax-induced surplus. In fact, the funds for this subsidy previously were being used to provide government services and we might therefore consider that the reduction in income is distributed in proportion to the enjoyment of services which have been reduced.

Conclusion

This analysis has estimated the effects upon the distribution of personal income of alternative monetary and fiscal actions to stabilize the general price level.

The long-run estimates, based upon the distribution of asset holdings and liabilities among income classes in 1957, show the following:

1. An uncountered price increase would have reduced the average real net worth in the income classes that receive less than $6,000 and increased the net worth in the classes above $7,500. The $6,000-7,499 class experienced a slight loss under the first assumption about the distribution of federal debt and a gain under the other.

2. An increase in interest rates to counter inflation would result in a net gain in receipts for classes receiving less than $6,000 of personal income (under Assumption I) with a net increase in payments for incomes above that level. Under the second assumption there is a slight net increase in payments for the $5,000-5,999 class.

3. A tax increase would cause progressive reductions in disposable income with maximum progressivity under a corporate income tax increase. There would be a partially compensating decrease in the interest burden upon the upper income classes, since the increased tax receipts and purchase of debt would make interest rates lower than would otherwise be the case.

The short-run, structural effects from the induced reductions in demand would result in the following losses in employment and in wages and salaries, under the assumptions about immobility of resources described earlier:

1. An investment reduction caused by an interest increase of 1.7 per cent would have reduced employment by 1.6 per cent of total man-hours in 1957 and caused a loss in compensation of employees of 4.4 billion dollars, or 1.9 per cent.

2. A reduction in consumer expenditures of 9 billion dollars due to a tax increase of $13.00 would have reduced both employment and compensation of employees by 1.3 per cent of the 1957 levels.

3. A comparable reduction in 1957 federal spending levels without a tax decline would have caused employment to decline by 1.5 per cent of total man-hours and compensation to fall by 1.8 per cent.

DISCUSSION

ROBERT W. OZANNE: Mr. Weintraub has given us both a very fine analysis of our present dilemma and a plea for a revised distribution theory which will better enable us to cope with current economic realities. In addition, from the background of his own research and writings he has suggested some guideposts for such a revision. With his analysis and with his suggestions for revision I find myself in quite general agreement.

Because of the broad range of Mr. Weintraub's subject matter and my very limited time, I have chosen for analysis one small section of his relative shares discussion entitled, "A Capital Output and Wage Share Truism."

Let us look first at what Mr. Weintraub calls his price level truism, $P = k \, w/A$, which simply states that current dollar output per worker divided by constant dollar output per worker equals the price index. What makes the truism interesting is Professor Weintraub's use of the constant, k, meaning average business markup over wages, to arrive at current dollar output per worker.

Where did the constant, k, come from? In Mr. Weintraub's recent book, *Forecasting the Price Level, Income Distribution, and Economic Growth*, this k was derived from Department of Commerce data by dividing business gross product by compensation of employees. This was done for the years 1929-57 and a markup factor, 2, was selected as the constant. This figure of 2 means that the gross business product per worker was double the labor share or the very interesting conclusion that the labor share since 1929 has stayed so close to 50 per cent that it can be considered a constant.

On the basis of this apparent constant, Mr. Weintraub in his book went on to advocate a wage policy consonant with stable prices.

Just what is the appropriate use of empirical "constants" in economics? Mr. Weintraub asks the same question in a footnote in this presentation. He says:

> It is rather distressing to find so much discomfort over the various evidences of certain important constancies in economics, in saving ratios, capital-output ratios, productivity developments, and not least, the labor share. . . . I have even encountered the twisted argument that the recognition of a (nebulous) functional relationship imparts more insight than the acknowledgement of an empirical (near) constant. . . . Does work with such fuzzy "functions" advance our stature as a science in the same way as empirical recognition and analytical utilization of (near) constants in other functional connections?

I certainly do not think we should kick constants under the table, but I am very skeptical of their existence or real usefulness. Before using a constant, I would want to understand why it is constant. If there is no theoretical explanation of a constant, it should be very suspect.

To return to k, or in this case its reciprocal, the labor share constant of 50 per cent of gross business product, we normally think of the labor share of national income as rising in deep recessions as profits vanish but as showing relative stability in the long run or perhaps increasing slightly as relatively more workers become employees.

Mr. Weintraub has uncovered a constant through both the short and the long run. This has been accomplished by calculating the labor share of the gross business product rather than of national income or of private national income. This means that Mr. Weintraub has added to the normal recipients of national income—labor and capital—government as a recipient of indirect business taxes and capital consumption. These two new nonlabor recipients grew relatively in the depression of the thirties, thus balancing the disappearance of profits and cutting down labor's normal rise in a deep recession.

Insofar as this accounting has minimized the short-term fluctuations, it is possibly desirable. In the long run, it probably will not conceal real share changes. Thus using Mr. Weintraub's method, the total nonlabor share in 1929 was 16 per cent above 1957; in 1950 it was 7 per cent above 1957; and in one year, 1955 to 1956, the nonlabor share fell 3 per cent. Hoping to avoid any quibble over what may be termed a constant or near-constant, I would label changes of the magnitudes just listed as significant relative share changes.

Even if k should turn out to be a near-constant, it would not be of practical value until we learned the autonomous factors which brought it about. Do wages push up costs? Do profits push up costs? Are they both pulled up by demand? These are the important questions, the answers to which will bring significance to a discovered near-constant.

If Mr. Weintraub were to buttress his empirical near-constant with some theoretical reasons as to why we can expect the wage share to remain constant, it would be more impressive. If this country were to move presently from the current recession to a high level of business activity, I see no theoretical reason, k notwithstanding, why the labor share of gross business product might not drop 3 or 5 per cent as profits zoom. Certainly without a theoretical underpinning, Mr. Weintraub's constant is of limited use in wage-price policy formulation—a usage for which he has already recommended it.

Now, let us turn to Mr. Weintraub's capital output and wage share truism. This second truism ($W_y = w_r \dfrac{K_y}{K_n}$) also deals with the wage share. As Mr. Weintraub puts it:

. . . the wage share is equal to the real wage, multiplied by the capital-output ratio (K_y), divided by the magnitude of capital per head, K_n.
. . . .
Also, it is not surprising that in an economy in which K_y [the capital-output ratio] is largely constant that W_y [the wage share] will be highly stable. The two are immutably bound together.

Here I should like to ask why. Why should the constancy of the capital-output ratio have much impact on the stability of the labor share of gross business product? I should think that the degree of monopoly of capital and labor, the relative supplies of capital and labor, and, as a factor in gross business product, the government actions on business taxes and laws on depreciation policy would all be more important in their effect on the wage share than the capital-output ratio. In fact, I fail to catch the economic significance of adding to the

price level truism the stock of capital equipment, K, and its transformation into the wage share equation.

If we were to experience a period of high-level business activity, increasing monopoly in busines pricing, and tax revision to encourage depreciation, how would a constant capital-output ratio prevent a diminution of the labor share? Is not the capital-output ratio a neutral influence on the wage share?

In conclusion, I tend to agree with the main direction toward which Mr. Weintraub has pointed for distribution theory. On one point I have expressed some skepticism as to the existence and use of empirical constants in economic data.

Boris P. Pesek: Bertrand Russell once wrote of his discussions with Lord Keynes, "When I argue with him, I feel that I am taking my life in my hands, and I seldom emerge without feeling something of a fool." I cannot deny that I approach discussion of the paper written by Professors Brownlee and Conrad with very similar feelings.

The paper covers a virgin territory. The authors do not quote, and I am not aware of, any other writings which explore the distributive effects of a tight money policy and compare these with the effects of several alternative policies. They evaluate the distributive effects of inflation as compared to those of several policies alternative to inflation: a tight money policy, a policy of increased federal taxes, and a policy of decreased federal expenditures. The authors conclude (1) that inflation exacts a sacrifice from those with incomes below $6,000 and gives a bonus to those with incomes higher than that; (2) that tight money policy achieves precisely the opposite results; those with incomes below $6,000 benefit and those with incomes above this limit lose; (3) that increased federal taxes, regardless of which of the three tax alternatives is selected, will impose a progressive burden on all income groups; (4) that, finally, the effects of decreased federal expenditures will depend on the type of government activity affected by this cut.

Very surprising is the conclusion that inflation penalizes the low-income groups and benefits the high-income groups. In two other articles which explored this particular problem,[1] one conclusion was that all income groups are net creditors and that, therefore, all income groups suffer from inflation. My own view was that inflation is regressive only when compared with the effects of either income or sales taxation. In other words, I show relative regressivity. In Brownlee and Conrad's paper, inflation is absolutely regressive: it takes away from the poor and gives to the rich. The differences in results depend on the basic data on the net worth of various income groups used in these three papers and on the allocation of the net business debt and other items to various income groups.[2]

This brings us to the central core of Brownlee and Conrad's paper;

[1] G. L. Bach and Albert Ando, "The Redistributional Effects of Inflation," *Rev. of Econ. and Statis.*, Feb., 1957, pp. 1-13; Boris P. Pesek, "A Comparison of the Distributional Effects of Inflation and Taxation," *A.E.R.*, Mar., 1960, pp. 147-53.

[2] I use data for early 1950 from Raymond Goldsmith *et al., A Study of Savings in the United States* (Princeton, 1956, W-46); Brownlee and Conrad use data for 1957 derived from the Consumers' Union-National Bureau of Economic Research panel.

namely, to the quantification of the costs of a tight money policy. When reading the paper, I was immediately struck by a very, very remarkable fact. Tight money policy is said to benefit low-income groups while the tax alternative costs them money. High-income groups lose money as a result of a tight money policy but they lose more money as a result of any one of the three federal taxes which the authors consider. Thus, inevitably, the tight money policy is much less costly to the public than any one of the three alternative tax programs. Tight money policy emerges as a perfect alternative to the policy-maker: (1) it enables him to take much less of real disposable income than he would have to take away were he to select any of the tax alternatives; (2) no legislation is necessary and the burden of the anti-inflationary policy can be hidden in the price structure and interest receipts, while increased taxes are only too painfully obvious to each voter; (3) instead of taxing all spending units by following one of the three federal tax methods, the policy-maker can actually benefit groups with incomes below $6,000, which include 32 million spending units, and can place upon the remaining 19 million spending units a burden which is smaller than would be that of any one of the three tax alternatives.

This converts the task of fighting inflation into one of the most enjoyable activities. Any President would be well advised to create inflation just to give himself the pleasure of fighting it. It sounds too good to be true and, indeed, I want to argue briefly that it is not true. To measure the cost of a tight money policy, Brownlee and Conrad calculated what they call in their table, "Net Change in [Interest] Receipts Minus [Interest] Payments." The table contains the increase in mortgage and consumer credit payments, increases in interest payments on public and private debt, and increases in interest receipts for bonds, deposits, pensions, and so on. But this surely does not express the entire cost of a tight money policy. In what way are we to obtain a rate of interest sufficiently high to mop up all the excess aggregate demand? Only by decreasing the quantity of money can we accomplish this goal. Indeed, in the introduction to their paper the authors state clearly that "increases in interest rates [are] brought about by a reduction of the supply of money or as a result of changes in the willingness of individuals to hold debt. . . ." In the empirical part of the paper, the authors lost sight of this fact.

The decrease in the quantity of money is surely another of the costs of a tight money policy. It is a cost no different from the cost of inflation which the authors explicitly calculated. In the case of inflation caused by an autonomous increase in demand, as specified by the authors, the nominal quantity of money remains constant, the price level increases, and thus the real quantity of money decreases. This is the cost of inflation so far as the money assets of various income groups are concerned. In the case of a tight money policy designed to combat this inflation, one cost is the changes in the net interest receipts resulting from higher interest rates. These changes the authors have quantified in their table. However, a rate of interest sufficiently high to mop up inflation can be obtained only by a decrease in the quantity of money. In this case the price level remains constant, the nominal quantity of money

decreases, and therefore the real quantity of money decreases. This is a second cost entailed in a tight money policy: the distributive burden of it will depend on the quantity of money which must be destroyed and on the distribution of this quantity among the various income groups. This second cost of a tight money policy the authors have neither considered nor quantified. It seems certain that had they done so the peculiar results which they obtained—namely, that the aggregate burden of a tight money policy can be substantially lower than the aggregate burden of taxation of equal anti-inflationary potency— would have been wiped out. The aggregate amount of impoverishment of the spenders necessary to combat a given degree of inflation can be but marginally different, regardless whether we select the policy of taxation or the policy of tight money.

Finally, the increase in the rate of interest will cause the prices of outstanding bonds to fall; consequently owners of interest-bearing assets already outstanding will experience capital losses. These losses, once again, represent an additional cost of the tight money policy which does not find expression in the tables prepared by Conrad and Brownlee. To express the full cost of a tight money policy and to make the calculation of the distributive effects of a tight money policy comparable with the calculations of the distributive effects of either taxation or inflation, the authors should add to their calculations of the gains and losses experienced by various income groups as a consequence of changes in net interest receipts the capital losses due to the decreased quantity of money held by various income groups and the capital losses due to the lower prices of outstanding interest-bearing assets. This would be unnecessary only if the demand for money were perfectly interest-inelastic. However, in such a case, the autonomous increase in the demand for goods and services— with which the entire analysis started—could not lead to an increase in the price level and the problem of selecting the best tool with which to fight inflation disappears: there can be no inflation.

While this criticism, if correct, affects seriously the estimates of the distributive costs of a tight money policy, it leaves unaffected the very valuable calculations of the distributive costs of inflation and of various forms of federal taxation which form another part of Brownlee and Conrad's paper. While the quantitative differences between the calculations of the burden of inflation and federal taxation found in my own paper and that of Brownlee and Conrad are not insignificant, it is encouraging to note that that qualitatively our conclusions are identical. Both analyses show inflation to be more regressive than federal income taxation. The Brownlee-Conrad paper shows inflation to be more regressive than either a proportional increase in all federal taxes or an increase in federal corporate taxes. My own paper shows inflation to be more regressive than two types of federal sales taxes: those excluding food and those including food. Since Brownlee and Conrad's paper shows inflation to be more regressive than does my paper, this latter conclusion must be true, a fortiori, when we use their estimate of the distributive costs of inflation and my estimate of the distributive costs of sales taxation. If distribution of income is our worry, the case against inflation is beginning to look quite strong. The relationship between economic growth and inflation,

unfortunately, continues to complicate the picture. Brownlee and Conrad, too, felt is necessary to add a word of caution with respect to this part of the problem.

There are a few other points which I would like to make before closing. In this paper the authors employ a multiplier relating spending and income to calculate the distributive burden of a tight money policy; or rather, in view of my discussion, that part of the total cost of tight money policy containing the net changes in interest payments. They use a multiplier of 1.4; in this they might be unduly conservative. In the recent paper by Friedman and Meiselman, covering the period from 1897 to 1958,[3] not a single one of the fourteen subperiods considered has a multiplier lower than 1.5; all but one have multipliers higher than 2.5; for the most recent period 1945-58 Friedman and Meiselman have obtained multipliers ranging from 4.7 to 8.2. The use of a higher multiplier in Brownlee and Conrad's paper would further decrease the interest portion of the cost of a tight money policy. Finally, when the authors estimate the burden of decreased federal defense expeditures, they are allocating this burden of smaller protection equally to all citizens. I suspect that there are vast differences in the opinion of, say, Earl Browder and Barry Goldwater as to the benefits received from the military protection of the United States. Whether these differences depend on the level of personal income and thus affect the conclusions of the authors is open to doubt. Browder would surely say that they do depend on income; Goldwater would deny it— as do Brownlee and Conrad.

By connecting these four names, I might be really "taking my life into my own hands." In addition, the attention span of this audience—as estimated in advance by the organizers of these meetings—is coming to an end. I might close by expressing my conviction that the paper presented to us by Professors Brownlee and Conrad is exceedingly significant for those interested in the distributive effects of various anti-inflationary policies and merits much closer scrutiny than was possible in this brief space.

[3] Milton Freidman and David Meiselman, *The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897-1958*. (A preliminary report to Commission on Money and Credit, 1960.)

CAPITAL THEORY

CAPITAL THEORY AND SOME THEORETICAL PROBLEMS IN DEVELOPMENT PLANNING

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One might easily argue that economic development planning is no more than an exercise in the theory of capital, for after all, capital theory is the theory of the time structure of production, which is the problem of development planning. But this statement is not true at a meaningfully low level of abstraction. In fact, as it has been developed by Boehm-Bawerk, Wicksell, Hayek, the Lutzes and others, traditional capital theory has been concerned with such problems as the definition of real and monetary capital, the determination of the rate of interest, the average period of production, widening versus deepening the capital stock, and certain marginal conditions for investment decisions. It would indeed be putting new wine in old bottles to treat the theory of development planning in the traditional framework. On the other hand, the logical problems associated with the time structure of production are sufficiently knotty, that it would be rash indeed to suppose that the classics have nothing to teach us, or that we can redevelop the correct solution to old problems as a sheer exercise.

This paper begins at the beginning of a theory of development planning. Particularly three problems will be considered: (1) the definition of an objective function and the determination of the rate of total investment, (2) the present state of knowledge about the necessary macroeconomic relations between investment and growth, and (3) additional possible methods of deriving estimates for this relationship.

I. *The Objective Function and the Choice of Level of Investment*

Determination of the proper total level of investment is perhaps the most difficult theoretical problem in planning. It is the normative—or welfare economics—side of capital theory. At least four solutions have been advanced.

1. *The "Classical" Solution.* This calls for investing the amount which is saved voluntarily in the country. Under conditions of perfect foresight and a perfect capital market, the interest rate will equate marginal rates of substitution in production and consumption among periods, one of the necessary conditions for an optimal growth path.

This solution had not been advanced seriously in many years until recently, when it was advanced in the recent American controversy about economic growth as a contrast to the repugnant idea of "forced growth." Pigou rejected it on the grounds of the shortsightedness—or irrationality—of individuals. Others have stressed the imperfections in the capital market evidenced by the typical wide gaps between lending and borrowing rates of interest, and the ethical question of the distribution of welfare among generations. Capital accumulation has also been viewed as a collective good, in which individuals may be willing to save more if they can be sure that others are also forced to save. This phenomenon can be derived from individual preferences about the value of consumption of contemporaries versus future generations, as has recently been shown by Sen and Marglin, or from the assumption of increasing returns to investment over the relevant range. Business saving, particularly where management is separate from ownership, also does not correspond to the classical assumptions, unless the managers do no more than carry out the wishes of the stockholders. Finally, with a large government, fiscal and monetary policies inevitably affect the total rate of saving, and it becomes difficult to define, much less to abide by, public policies which are neutral in this regard. Thus, each country, willfully or by default, must choose a rate of investment and a rate of growth.

2. *Utility Theories of Intertemporal equity.* Once one abandons the market solution, the problem becomes rather similar to an equity problem in taxation: what is the proper distribution of income over time? The utilitarian approach of Ramsey has recently experienced a revival at the hands of Tinbergen, Harrod, Stone, myself, and others, who have postulated a marginal utility of income function, combined it with a simple theory of the growth of output, and determined an optimal rate of saving, investment, and growth.

These theories are open to the same criticisms as the utility approach to determining optimal tax rates. Interpersonal comparisons of utility —even over time—have to be assumed, as well as cardinality. More fundamentally, since there is little empirical evidence on the shapes of individual utility curves apart from Frisch's data, the utility function has to be assumed, and so, in effect, the result is assumed.

In the case of the investment rate, the utility approach is somewhat more useful than in taxation. There is more room for analysis, for more economics between the assumed utility function and the solution. Such factors as population growth, departures above subsistence levels, trends in consumption in the emulated societies, as well as an empirical model of the effect of the rate of investment on growth can be explored, and interesting, not obvious, results can be obtained from

plausible utility functions. A utility function—more properly a social welfare function—also can assure some consistency in different policy measures, ruling out clearly inefficient combinations. For example, it might exclude extreme present sacrifices for the sake of growth-facilitating policies in some sectors, while in other sectors provision for growth is treated quite lackadaisically. It can also provide a check on pragmatic criteria like pay-off periods. Nevertheless, this approach leaves something to be desired, since its point of departure has no empirical basis.

3. *Fixed Growth Objectives*. Another approach in some current vogue is to specify a fixed growth objective and to compute the rate of investment necessary to achieve it from empirical data. Where some minimum growth rate is a dominant objective of economic policy, perhaps because of high population growth and the need to provide the people with at least some noticeable increase in their real income, a country may use this technique. Not much can be said about it since it essentially assumes that the growth choice directly is made at the political level, with consideration of the economic benefits and costs of alternative rates. Also, as we shall see below, we are still on very weak ground in estimating the investment rate necessary for any growth objective.

4. *The Doctrine of "Full Potential" Growth*. In an important recent article, Branko Horvath,[1] a Yugoslav planning expert, has advanced the idea that maximum growth is optimal. Besides some implicit theorizing, Horvath advances two empirical propositions: first, for every country, there is a maximum rate of investment which can be usefully absorbed by the economy, beyond which the marginal efficiency of investment becomes negative; second, the gain in output from any increase in investment short of that maximum point is so great that the gain from the powerful compound growth rate will outweigh any near-term losses. Horvath thinks that for many countries the maximum rate of investment is on the order of 30 per cent, and that it can yield a rate of growth on the order of 10 per cent. The limits to investment are set by the absorptive capacity of the economy, which in turn is fixed by the rate of increase of consumer desires, by the state of health of the workers, by the degree of knowledge, and by the state of economic and political organization. If the investment level is too high, the planning mechanism breaks down; bottlenecks develop; timing becomes too critical; the labor force is not able to operate and maintain the additional capital; requisite social changes become too great, and the need for innovational management outruns the potential supply of such talent in the population.

[1] Branko Horvath, "The Optimum Rate of Investment," *Econ. J.*, Dec., 1958, pp. 747-67.

No doubt Horvath overstates the case. At subsistence levels of consumption, the withdrawal of a few per cent of output from consumption does pose a difficult choice. The switch from increasing returns to negative returns cannot come at a point. And the absorptive capacity is itself a variable subject to policy, certainly through public expenditures in education and health. But Horvath's fundamental point cannot be shrugged off, even if it is contrary to our professional preferences for optimal rates of balance at some margin.

What Horvath suggests is this: each economy has some growth potential. It is foolish if it does not exploit it to the fullest. If the plans overreach the potential, the country will soon find it out and it will be forced to cut back. Thus, a meaningful set of instructions to development planners may be to plan for maximum growth, to seek out all genuine investment opportunities, but also to make sure that the human, technical, intellectual, and organizational resources are sufficient for the plan. Whether conflict with other economic objectives, such as price stability, equity in the distribution of income, or balance in the international accounts will in fact arise is something to be determined only after the investment potential is explored. And in the event of conflict, one might argue that fiscal, monetary, and foreign borrowing policies must then be devised which reduce the conflicts without having the country depart very far from its full potential growth path.

Horvath's argument also should teach us this lesson: an analysis of the proper rate of saving and growth gets nowhere without some knowledge about the production possibilities side of the question.

II. *Macroeconomic Planning: Investment and the Growth of Output*

How can development planners today estimate the relation between the investment rate and the rate of growth of output? First, where a country has a history of a planned development effort, it will have detailed industry data which allow it to do its planning without establishing macro relationships. Correct planning requires that scarce factors, like capital, foreign exchange, or trained managers, be allocated on an economy-wide, or at least plan-wide, basis, but this can be done by the use of shadow prices, without a macroeconomic model.

However, some countries have found it useful to use a macro-economic projection as a first step in drawing up a plan. This permits a rough estimate of the total potential supply of output, both in order to discover the total resources available as the plan proceeds and to assure that demand is allowed to grow at an adequate, yet non-inflationary, rate.

Traditionally, incremental capital-output ratios have been used for this phase of planning. The growth of output to be expected from the

total capital accumulation, or conversely, capital requirements for given output targets, can be estimated.

However, this method involves too large a margin of error. For example, if a figure of 3.0 for the capital-output ratio is used when 4.0 proves to be the correct one, then the rise in output is overestimated by 32 per cent after one year, and by rising percentages thereafter if the rate of investment depends on the increments in output. This margin of error is too large, leaving a range from inflationary shortages to slack utilization of resources well within the possible outcomes of a fixed development plan.

More recently, aggregate production functions, pioneered by Douglas and given a modern revival by Solow, have come into use for projection in advanced countries. Statistically, these functions "explain" the historical changes in output with astounding precision, and while methodological objections can be raised, their success should preclude the use of even cruder techniques. In Table 1, two hypothetical forecasting tests are made, using the modernized Cobb-Douglas production function derived by Knowles for the U.S., and comparing it with projections made by assuming constant capital-output ratios. Five-year intervals—a common period for development planning—are forecast, using change in "potential" output, an output measure adjusted for cyclical swings, as the criterion. It can be seen that the production function forecasts better.

But the production function approach suffers from some limitations of its own. First and most important, because the time series of factor inputs all have strong trends in them, they are multicollinear. Thus, in particular, it is difficult to isolate any interaction between capital accumulation and the time trend which stands for technological change, and no interaction is assumed. The functions also assume constant returns to scale and high elasticities of substitution between factors.

Solow[2] has recently made some estimates of the possible magnitudes of the positive interaction between investment and technological change. On the basis of the changing age structure of capital and hence of the date of the average technology in use, he concludes that if capital is given full credit for the joint effects, its impact on growth of output might be as much as twice as great as the direct impact of capital alone.

For making projections in an advanced economy, this range of uncertainty may not be a crucial matter, since the trends can be expected to persist and the rate of capital accumulation to continue not too differently from past rates. Unless the economic structure changes—which no

[2] Robert Solow, "Investment and Technical Progress," in K. J. Arrow et al., eds., Mathematical Methods in the Social Sciences, 1959 (Stanford Univ. Press, 1960), pp. 89-104.

formula based on historical data can allow for—the aggregate production functions should do very well, better than the incremental capital-

TABLE 1

TEST OF ALTERNATIVE PROJECTION TECHNIQUES
(In Billions of 1954 Dollars)

(A)	(1)	(2)	(3)	(4)	(5)
Period	Change in Actual Output Adjusted for Cyclical Factors	Predicted Change in Knowles' "Potential" Output	Predicted Change on the Basis of Constant Capital-Output Ratio	Predicted Change on the Basis of Average Capital-Output Ratio for Past 5 Years	Predicted Change on the Basis of Incremental Capital-Output Ratio for Past 10 Years
1913–18................	9.6	13.7	19.5	18.0	—
1918–23................	23.8	18.0	20.2	17.0	12.2
1923–28................	26.9	32.2	20.1	17.0	25.7
1928–33................	13.4	17.8	14.4	12.9	14.4
1933–38................	28.4	23.3	2.2	2.0	2.0
1938–43................	33.3	40.6	8.3	8.1	20.7
1943–48................	43.7	33.9	29.9	31.9	310.5*
1948–53................	66.0	63.8	51.9	62.3	104.7
1953–58................	62.6	82.9	53.0	62.3	65.0
(Average per cent errors).		(24.2%)	(42.9%)	(39.8%)	(33.8%)*

Col. (1): GNP (in billions of 1954 dollars) adjusted for cyclical variations using J. W. Knowles' time series "O_m" and "O_p" (using the ratio O_p/O_m as cyclical adjustment factor). See J. W. Knowles, "The Potential Economic Growth in the U. S." (Joint Economic Committee: *Study of Employment, Growth and Price Levels.* Study Paper No. 20, Jan. 20, 1960.)

Col. (2): Knowles' estimates of "potential output" on the basis of a production function.

Col. (3): Assumes constant capital-output ratio: 1909–58 average ratio of "actual output, cyclically adjusted" (Col. 1) to MAPI estimates of gross stock of tangible capital.

Col. (4): Change in stock of capital multiplied by average capital-output ratio of last five years.

Col. (5): Change in stock of capital multiplied by incremental capital-output ratio of last ten years.

* If the last twenty-year incremental capital-output ratio is applied to the 1943–48 prediction to allow for the depression, we get 55.0 billion instead. The average per cent error is applicable to the revised prediction.

output ratios. This can also be seen from more formal considerations. If a Cobb-Douglas function with a trend term fits the data, i.e., if

(1) $$x_t = aL^\alpha K^{1-\alpha}(1 + r)^t, \qquad \text{then}$$

(2) $$\frac{x_t}{K_t} = a\left(\frac{L_t}{K_t}\right)^\alpha (1 + r)^t, \qquad \text{or}$$

(3) $$\frac{K_t}{x_t} = \frac{1}{a(1+r)^t}\left(\frac{K_t}{L_t}\right)^\alpha,$$

where L is labor, K capital, x output, r the trend factor, t the period, α and a are constants. When capital grows at the same rate as labor,

the capital output ratio falls by the trend factor. If capital grows faster than labor, this will tend to make the capital-output ratio rise; which of the two elements dominates, technological advance or capital deepening, depends on the empirical magnitudes.

In the U.S., over the last forty years, capital has grown at 2.4 per cent, man-hours at 0.9 per cent and the trend appears to have been 2 per cent or so. According to equation (3) the capital-output ratio should have fallen by 0.5 per cent a year, or a rough total of 20 per cent. In fact it fell by about 25 per cent (Kendrick's data as reported in Knowles).

Where capital accumulation is very high, the rise in the capital-labor ratio will exceed the technological trend rate. For example, in Russia, where the capital-labor ratio must be rising by something like 5 per cent a year and the technological trend does not appear very different from the U.S., the capital-output ratio should be rising if a Cobb-Douglas function applies. This is the actual pattern.

While the production function approach can be used to prevent the mistake of assuming constant capital-output ratios and perhaps to make projections, so far it has not been worked out with sufficient precision to determine the rate of investment in policy analysis. The typical function for the U.S., such as that of Knowles, implies that an increase in the rate of net fixed capital formation (excluding government and housing) by 50 per cent from 20 billion to 30 billion a year, would mean a difference in the rate of growth of only 0.42 per cent, or expressing this as a rate of return, the extra 10 billion yield 2.1 billion, or 21 per cent. But using Solow's crude estimates for the interaction between investment and technological change, these figures double. If further allowance is made for an elastic labor supply with rising wages and for the higher level of demand likely to be associated with a higher rate of investment, the total direct and indirect effect of investment on growth becomes even greater. On the other hand, we have no evidence on the absorptive capacity of our economy. Thus, in considering the effect of raising the rate of investment on the rate of growth of output in the context of a planning model, the uncertainty on the production side is still great, even for a country with the statistical resources of the U.S. For projection purposes, the large magnitude of the trend term in the aggregate production function is useful, but for policy purposes it simply indicated the vastness of our ignorance. Hypotheses such as Horvath's cannot be subjected to empirical tests with present methods.

III. *Possible Other Approaches*

Since aggregate production functions so far have not yielded reliable estimates of the relationship between investment and growth, one

must try other methods. I shall mention four possible sources of evidence.

First and most important are empirically derived cost functions, testing whether or not increasing returns to scale prevail in various industries. For if there are increasing returns, the achievement of a higher rate of investment permits the exploitation of these economies. The question has been discussed for decades and on the answer hinges much of development strategy. Balanced growth, leading sectors, big push, take-off, industry versus agriculture, export development versus import substitution and so on in large part are hypotheses which depend upon the presence or absence of increasing returns. Our knowledge is meager. Most recently, Chenery[3] has argued that economies to scale do prevail in such industries as machinery, transport equipment, and in certain other industries producing goods heavily traded internationally. Salter's[4] study of manufacturing industries in England also suggests increasing returns. And the work of Isard[5] and others has shown that the minimum scale of economic operation in some industries is so large that economies of scale would be realized over the relevant range in all but the largest developed economies. In the context of underdeveloped countries, these technical economies of scale may be outweighed by diminishing returns caused by inadequate supply of certain scarce factors, particularly by the supply of trained manpower and managerial capability. In this situation, a program for developing the requisite human resources is necessary. In fact, it may well turn out that the answer to the often-asked question of the relative returns to human and physical capital may lie not in considering them as alternative investments but in treating them as complementary. The education effort must suffice to meet the skill requirements associated with an investment program big enough to realize the economies of scale.

A second approach utilizes the data from individual projects. Much of present development planning separates the project planning function from development planning. The central planner takes the projects as given and views his own task merely to select among the projects submitted. Using his industry and commodity statistics, he then strives for consistency and co-ordination. As a result, crucial incremental choices about scale, capital intensity, the use of foreign exchange and of managerial talent may be made in ways which do not reflect the social scarcity values. Further, if the central planner does not himself come in contact with the details of production functions of individual projects, he cannot pass a reasonable judgment about the presence or

[3] H. B. Chenery, "Patterns of Industrial Growth," *A.E.R.*, Sept., 1960, pp. 624-54.
[4] W. E. G. Salter, *Productivity and Technological Change* (Cambridge: Univ. Press. 1960).
[5] Walter Isard, *Methods of Regional Analysis* (Wiley, 1960), especially Chap. 7.

absence of economies of scale. It should be possible, starting with detailed individual project data and tracing repercussion effects by appropriate econometric and structural analysis techniques, to see whether increasing or diminishing returns to investment prevail.

Third, the hypothesis of limits to the administrative capability of managing a plan above some critical magnitude must be tested. Horvath alludes to the Russian experience. But there may be other instances amenable to scholarly investigation, including examples of private corporations. Finally, the general record of economic history may be of relevance. A number of countries have succeeded in achieving rates of growth of 8 per cent to 10 per cent at some stage early in their development. The same materials which suggest the hypotheses of the big push and the take-off into self-sustained growth should also provide some insight into the relation between investment, economies of scale, and growth. But where the broader theories only require the coincidence between large efforts and successful growth, a development planning agency must have a more specific understanding of the process. Is it technological economies of scale? Or rising labor productivity under the impetus of a changed and changing industrial situation? Or the sudden application of a large stock of new technology? Or is it some other factor?

IV. Capital Theory and Expenditure Criteria for Development Planning

So far, we have not found much direct applicability of the theory of capital to development planning. I think the explanation is quite simple. Capital theory was developed in a day when there was little formal verification of the theories with the facts; that is, it was developed in the prescientific era of economics. Capital theory is not a theory of development; its formal structure and fundamental postulates do not permit enough of the reality of a developing economy to be introduced to yield directly fruitful results.

This is not to deny that in a much looser sense some ideas of capital theory would be useful for a development planner to know. Above all, he should appreciate the scarcity value of capital. And his planning system, even if it does not have the advantage of a perfect capital market and an equilibrium market rate of interest, must still somehow guarantee that capital—and other scarce resources—will be put into their highest uses. He should also be aware that investments can be postponed, that capital intensity is variable, and that the most durable project is not always the best project. He may find some of the marginal conditions derived by Lutz and Lutz of direct applicability to small investment choices. Also, as Professor Dorfman has shown,

there are some formal similarities between the management of water storage reservoirs and some chapters in classical capital theory. And he may even find some notions of the wage-fund doctrine useful in administering foreign aid under P. L. 480. But I think one would credit capital theory with too much if one attributed to it the solution to these and other emerging problems in development planning.

On one particular problem, which, for better or worse, has loomed much larger in the theoretical literature than in application, capital theory has a very direct bearing: the choice of formal expenditure criteria. The controversy about rates of return versus ratios of present value such as benefit-cost ratios or SMP is a pretty literal replay of the Lutz-Hildreth $V - C$ versus V/C controversy of the late thirties. I think the result of the recent version of that controversy can be summarized as follows: the form of criteria depends on the nature of constraint, the reinvestment possibilities and the form of the objective function. Except for very odd cases, the specification of intertemporal preferences is inevitable. A careful reading of Irving Fisher and of Lutz and Lutz would have brought that controversy to its conclusions more rapidly.

All in all, then, I find only a very limited relevance of capital theory, as typically understood, to development planning. I hope that the more humble empirical approaches discussed here will be useful to the planners that are making the irreversible choices that set the course of economic history for their countries.

SOME THEORETICAL ASPECTS OF
CAPITAL MEASUREMENT

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There are few areas of economic statistics in which progress is needed more and would be more valuable than in the measurement of capital. Capital measures are essential for analysis of investment demand, factor prices and inputs, production functions, and productivity. They are thus requisite to empirical study of economic fluctuations and growth generally and as a background for stabilization policy and development planning.

In this paper, I shall first pick out what appear to me to be the chief points in capital theory which are relevant to capital measurement. Then I shall examine critically what I believe to be false paths that have been taken in the direction of quantification. In an Annex to this paper[1] are set forth some positive suggestions for solution of several of the major problems confronting the statistician who seeks to measure capital in the way I believe to be most useful—as a weighted aggregate stock.

Basic Characteristics of Capital Relevant to Estimation

Capital as a Stock. The earlier classical economists tended to mix the notions of wealth as a fund and as a flow of goods. As Schumpeter points out, ". . . the classics were not very clear concerning the differences between funds and flows, and between wealth and the services of wealth."[2] He credits Irving Fisher in *Nature of Capital and Income* with nailing down the distinction and clearly defining capital as the stock of wealth of all kinds that exists at any moment. Fisher himself showed that the definitions of most classical writers really came to this. Most economists have continued to view capital as a stock, but not necessarily so comprehensively as Fisher.

The stock approach links the economist's capital concept with the accountant's capital account. This link has operational significance analogous to that provided by the Commerce Department's "rule" that national product consists of final goods and services defined as products which are not resold during the accounting period. The Fisher approach does not necessarily identify capital with those items included

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[1] This material was originally the last section of the paper, but space limitations preclude its publication. The Annex is available in mimeographed form from the author on request.

[2] Joseph A. Schumpeter, *History of Economic Analysis*, pp. 627-28.

in balance sheets of operating units, but the notion of capital as a stock of goods which can be inventoried and valued at successive points in time has obvious operational significance.

The inventory approach makes statistically irrelevant the Austrian concept of capital as an "intermediate product" temporarily embodying land and labor en route to ultimate consumption. The analysis of Boehm-Bawerk and his followers stemmed from a laudable desire to look beneath the surface and find a truly "genetic" explanation. But Schumpeter has observed that the Austrian theory is not relevant (as well as being nonoperational) "as soon as we realize that all economic theory is a theory of planning and inevitably has to accept the results of the past—plant, equipment, and stocks all included—*as data*. We shall then cease to try to construct an economic process *ab ovo* and, looking forward only, consider instead the 'amount of investment of capital.' "[3]

The role of capital must be distinguished from that of intermediate products that are themselves used up rather than just used in production during an accounting period. As Haavelmo has pointed out, "the trouble with the idea of capital as something that 'goes into' the product is . . . the result of a difficulty that perhaps most of us have of understanding how something can be used and render a yield in a process without itself being destroyed or depleted."[4]

To the extent that producers' durables are depleted in a period, in the sense of losing value, such capital consumption must be classed with intermediate product input.[5] The input of capital (human or nonhuman) must be thought of as a time-rate of use of stocks, valued in terms of their marginal contributions to revenue per time period. Even today, there is confusion between capital input and capital consumption.

The Scope and Structure of Capital. Some economists, like Fisher, would identify capital with wealth, including human resources. Others would confine capital to those resources or "factors" used in production. In common usage, many economists restrict the term capital to the produced means of production, on a par with land and labor as one of the productive factors.

Any broad groupings of wealth, or capital, and particularly the notion of a triad of factors, are misleading if the groupings are thought of as homogeneous quantities with unambiguous meaning rather than as "complex quantities." It was with this in mind that Frank Knight

[3] *Ibid.*, p. 908.
[4] Trygve Haavelmo, *A Study in the Theory of Investment*, p. 93.
[5] If the accounting period were very long, all "durable" but nonpermanent goods would become intermediate products. F. Hayek restricts the term capital to such goods (*The Pure Theory of Capital*, p. 54).

called the factor notion an "incubus on economic analysis." It is certainly true that there are many individual types of capital goods—and this goes for land and labor, despite the superficial appearance of homogeneity of acres and men. More fundamentally, all types of capital are really the same, economically, in that each provides a flow of productive services, and is valued thereby. Thus, in terms of valuation, capital is homogeneous; in terms of underlying physical characteristics, there are myriad varieties of capital goods which gradually change form in a dynamic economy.

Nevertheless, there is no inherent objection for purposes of theory or measurement in grouping capital into general categories possessing similar characteristics and exhibiting significant interrelationships. Most basic is the distinction between human and nonhuman factors (or capital). As Knight himself has pointed out, the distinction is fundamental in a free society in which men can sell their services but not themselves; whereas nonhuman capital goods are bought and sold, although owners may also lease the services of the durable goods. Because of the inevitably man-centered interests of man, we are also interested in the distinction with regard to the distribution of the national income, as conditioned by the relative inputs and rates of compensation of the two factors.

Certainly the distinction between human and nonhuman capital is essential for measurement. Records are available relating to the services and compensation of labor, but not to the value of the human labor stock since this is irrelevant in our type of society; records of both the compensation and value of the stock of nonhuman capital are available.

Within the classification of producers' capital the old distinction between land and produced capital has gradually broken down. The two are inextricably mixed physically and land can be "produced" when profit prospects warrant. More importantly, the two types of assets are valued in the same way.[6]

Construed broadly, capital should also include consumer durables and household inventories which also furnish a stream of services, although these are not generally valued through market processes except in the case of rentals of houses and a few other durables. Further, the required stocks in the business and household sectors are interdependent. Inventory fluctuations in the two sectors are obviously related, and the ownership of durables by households is reduced as leasing from the business sector expands.

This broad view of capital is inconsistent with the present U.S.

[6] Cf. Tibor Scitovsky, *Welfare and Competition*, p. 228: "From every point of view, therefore, land may be regarded as a capital good and the rent of land as similar in every respect to the gross earnings of a produced factor."

national accounts which measure current consumption in terms of purchases rather than the rate of disappearance of goods or their services. Adaptation of the accounts would involve a household investment account, the corresponding balance-sheet items of durable stocks and inventories, and estimates of the value of household capital services (including depreciation) to be included in the current consumption account.

There has been unnecessary argument as to the inclusion of social capital with private. As long as publicly-owned stocks contribute to the productivity of the private economy or furnish direct services to consumers, they must be counted. Otherwise, the amount of the community's capital will vary depending purely on the relative extent of public ownership. One of the criteria in national income measurement is that the flows should be invariant to purely institutional changes. The same rule is applicable to capital.

Finally, the concept of "intellectual capital" advanced by Adam Mueller, Friedrich List, and other early economists[7] has retained its vitality. This "intangible capital" comprises the technical knowledge or know-how of men as expressed in their activities, forms of organization, and tangible capital goods. It is the result of investments in the discovery and spread of productive knowledge.

Note that intangible capital is embodied in tangible capital both physically and in value terms. Factor compensation stems from both the physical and qualitative aspects of productive agents. It would require major statistical surgery to try to value intangible separately from tangible capital. The notion of intangible capital is an important one, as eloquently developed by Professor Schultz in his Presidential address, but I am skeptical as to whether it can be usefully measured.

Capital Value. Monetary values furnish the common denominator for diverse types of capital goods. Since the works of Boehm-Bawerk and Fisher, capital has generally been regarded as the discounted value of the future stream of revenue expected from capital goods. Thus it becomes possible to regard capital as a fund of abstract productive power. J. B. Clark, for instance, compared capital with a waterfall, which remains essentially the same even though the constituent drops of water are continually changing, just as the stock of capital, when maintained, remains a source of productive power in perpetuity, even though the capital is embodied in a succession of physical instruments.

Of course, any value aggregate can be thought of as representing physical goods or other "real" variables. The trick in converting a value fund to real terms lies in specifying the appropriate deflator which meaningfully corrects current values. It is here that a second

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[7] In England, Bentham, Senior, and Sidgwick, *et al.*, developed this approach.

aspect of the valuation process comes in: the prices of the underlying capital goods, as established in markets or imputed by owners, can be appropriately combined (with variable quantity weights) to provide a deflator to convert capital values into physical volumes of the various types of underlying capital goods at base-period prices. Or, the result can be achieved directly by weighting quantities by constant prices.

As I view it, this is the most meaningful way to measure "real capital stock," since the weighted aggregate measures the physical complex of capital goods in terms of its estimated ability to contribute to production as of the base period. Or, assuming that relative prices also approximate relative costs of production, the aggregate stock measures changes in real costs of capital goods at base-period levels of factor productivity.

The real stock estimates so conceived make possible comparisons with associated real output to obtain average productivity estimates indicating savings of capital per unit of output over time as a result of changing productivity efficiency and factor combinations. They also make possible the estimation of production functions and "total productivity" ratios indicating the net saving of resources per unit of output and thus changes in productive efficiency net of the effects of factor substitutions.[8]

Implementation of our concept of real capital stock, in a dynamic economy, requires working solutions to a number of difficult conceptual problems such as quality change, depreciation, and weighting. In view of these complexities, certain analysts have suggested alternative methods of measuring the physical volume of capital. Some of these less direct methods have been promoted as being not only simpler, but also more profound. As Joan Robinson has noted: "The human mind is naturally poetic and thinks in terms of mystic essences. The proposition that everything is what it is and not another thing, has to be accepted, but it goes against the grain."[9] We shall now critically review several indirect approaches to measurement of real capital stocks.

Indirect Approaches to Real Capital Measurement

Capital as Embodied Labor, or Real Cost. Many economists have been led by the deceptively homogeneous appearance of the worker, or man-hour, to try to measure capital (as well as output) in terms of the labor time embodied, or commanded.[10] Despite the statement by Mrs. Robinson just quoted, rather than measure capital directly in

[8] These concepts are developed in more detail in the author's study for the National Bureau of Economic Research, *Productivity Trends in the United States.*

[9] Joan Robinson, *The Accumulation of Capital*, p. 20.

[10] "Embodied" and "commanded" labor differ in movement insofar as wage rates in the capital goods sector move differently from those in the whole economy, and as the ratio of labor to total cost changes.

terms of what it is, she prefers to express it in terms of labor-time. She thinks this is easier than trying to measure "an enormous who's who of miscellaneous items that can be treated as a quantity only when it is measured according to some more or less arbitrary convention. . . . We can divide the value in terms of commodities of the stock of capital in any economy by the wage per man-hour in terms of commodities ruling in that economy and so obtain the quantity of capital in terms of labor time. This is in some ways the most significant way of measuring capital, for the essence of the productive process is the expenditure of labor time . . . capital goods in existence today can be regarded as an embodiment of past labor time to be used up in the future."[11]

The first weakness to note in Mrs. Robinson's argument is that not just capital but most economic aggregates comprise many heterogeneous and qualitatively changing types of units. With respect to labor force and labor time there are thousands of different types of occupational specialties, with different levels and changes in rates of compensation. To develop an average wage rate with which to deflate asset values, labor time weights are needed which immediately poses the index number problem. Thus Mrs. Robinson's procedure does not avoid the ambiguity of a weighting convention any more than did Keynes's attempt to state national product and its components in terms of wage units. For to convert man-hours of all types into common labor hours requires choice of relative wage rates, which differ somewhat depending on the period used.[13]

The more fundamental objection to this approach is theoretical. The original factors involved in producing capital may be labor working on natural resources, but, since Adam, man-made capital has also been used in the process, giving rise to capital compensation reflecting its productivity and scarcity. Even from the viewpoint of embodied resources, the man-hours commanded by the capital compensation part of asset values has no meaning and in a dynamic economy certainly does not show the same movement as the underlying capital stocks and input.

This leads to the final objection to this type of measure, whether looked on as an artificial conversion of asset values to man-hours or as the total real cost of producing the capital stock with current technology. In a progressive economy, the embodied man-hours or total real factor input will rise less than the real capital stock measured in its own units. This is enlightening when we wish to measure the

[11] *Ibid.*, pp. 22, 121.
[13] Cf. Alvin Hansen, *A Guide to Keynes*, p. 44: "Keynes' analysis could have proceeded quite as well had he adopted the price index as his deflator instead of his wage unit. . . . On balance, Keynes readers would probably have preferred constant-value dollars to constant-wage-unit dollars."

changed productivity in capital goods production; but is real cost a meaningful measure if we wish to learn the extent to which changing technology has been capital-saving (or using)? By relating capital-as-input to output, the effects of partial productivity advance are double-counted: once in the use of capital stocks as such to produce output and again in the use of labor (or total input) to produce the capital.

It is analagous, in measuring man-hours per unit of output, not to count the man-hours actually expended but rather the man-hours that would be required to produce a standard subsistence for the workers involved.[13]

Capital as Capacity. It is useful to have measures of output capacity, and some theorists have suggested measuring capital stocks in these terms, assuming "an appropriate amount of employment."[14] This approach has been seen as a possible way of handling the problem of quality change, since if a new model of a machine can produce twice as much output as the old model, it can be counted as twice as "much" capital. A more sophisticated version of this approach would relate the quantity of a capital good to its real marginal value product. We shall look at this variant later.

For the economy as a whole, or broad sectors, it is obvious that the index number problem is not avoided by the average capacity approach, since real capital would be approximated by estimates of the capacity of capital goods to produce a heterogeneous collection of outputs. There would be further measurement problems in a realistic situation in which part of capital resources were idle at times: An estimate of output capacity would involve guessing the average productivity of the unused resources, which would depend on the composition of the required additional demand in relation to the composition of the unused capacity.

Even if total output capacity of the capital stock of the economy were estimated as the sum of capacity of individual producing units at the most efficient rates of utilization—regardless of whether the capacity fully meshed, sufficient complementary resources were available, or whether capacity conformed to the actual structure of demand—there would be other problems. In particular, the "most efficient rate" of operations is an economic concept, depending on relative prices of inputs. This means that capacity output is difficult to estimate in the first instance, and that, in a dynamic economy, capacity (at the most efficient rate) changes as relative prices change.

Useful though capacity estimates would be in their own right, much

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[13] The same problem is also present, although less acutely, under conditions of unchanging technology, so long as the law of variable proportions operates. This provides another objection to Keynes's wage-unit.

[14] Robinson, *op. cit.*, p. 119.

would be lost by substituting them for estimates of real capital as such. By this convention, output-capital ratios would merely indicate changes in rates of utilization of capacity, and the marginal capital coefficient by definition would be unity. Further, as Edward Denison has pointed out, the results for capital formation are absurd.[15]

The alternative of measuring the quantity of capital in terms of the capacity of the capital goods to contribute to real income does take account of the freeing of other resources. That is, not only would the gross output capacity of the equipment be considered, but also the real noncapital costs of operation, the difference representing the contribution to real value-added. The real value of the capital goods would, of course, involve a discounting of the future annuities as defined.

Despite virtually insuperable estimation problems, this approach has been considered theoretically attractive. On this basis, a constant capital stock and zero net capital formation would maintain the productive capacity of the economy and thus provide a way to implement the notion of "keeping capital intact."[16] Essentially this method of measuring the quantity of capital goods, as a means of allowing for quality change, has also been suggested as the proper basis for pricing.[17]

The associated prices would, however, be quite artificial. After all, prices of capital goods in a progressive economy, assuming constant output prices, do not represent the present value of the future net income stream based on constant prices of inputs as well as outputs, but rather on a net income stream being narrowed with time as input prices rise relative to output prices as a result of technological progress. In other words, the capacity of new capital goods to produce real value-added (at constant output and input prices) is greater than their ability to produce real net revenue at constant output prices but with input prices rising as is typical in a progressive economy. Furthermore, since competition among sellers and buyers of the capital goods tends to push their prices and capitalized values down to equality with average cost, real costs are a better basis upon which to estimate relative physical volumes and prices (and provide a means of solving the problem of quality change, developed in the Annex).

As Denison stressed, "the idea of a 'capital saving invention,' for example, would have only the most limited applicability if the measurement of the quantity of capital were tied to its productive ability."[18] By this approach, the capital-output ratio would rise if, as is usual, innovations were laborsaving. Yet the physical volume of capital defined

[15] Edward F. Denison, "Theoretical Aspects of Quality Change, Capital Consumption, and Net Capital Formation," *Problems of Capital Formation*, Vol. 19, *Studies in Income and Wealth*, p. 229.

[16] *Ibid.*, p. 231.

[17] Preliminary Report of the Price Statistics Review Committee, NBER.

[18] Denison, *op. cit.*, p. 223.

as standard units in terms of base-period cost might well be falling relative to output.

Other indirect methods of measuring capital stocks have been suggested, such as dividing the value of capital assets by an index of consumer goods prices in order to measure the volume of consumption foregone to provide capital.[19] But none of the indirect approaches avoids the complexities of the problem, and all suffer from the defect of measuring something other than capital inventory as such.

Conclusion

All the alternative measures we have discussed would show the same rates of change in a hypothetical state of competitive equilibrium, with perfect knowledge, with technology and tastes constant, assuming proportionate and steady growth of the real capital stock and labor force. As soon as technological progress is introduced, the measures diverge. The physical capital stock rises more rapidly than the volume of labor or total inputs required to reproduce it at current technology; the physical stock rises less rapidly than its average output capacity or its capacity to contribute to real value-added; and physical stock rises either more or less rapidly than its real value in terms of consumption goods and services depending on relative productivity changes in the two sectors.

Measurement of capital as a weighted physical stock, which we consider the most useful general approach for reasons noted earlier, raises several major conceptual problems discussed in the Annex and elsewhere.[20] The first is the problem of measuring physical volumes of various types in standard units over time, when in fact the characteristics of most capital goods gradually change, new goods are introduced, and some goods have no standard unit. The second problem, distinctive to reproducible durables, is how to handle the depreciation or "consumption" of capital goods as they age. In addition, interpretation of the meaning of capital stocks in either current or constant dollars is clouded somewhat by the fact that in the real economy the meaning of price deviates from its meaning under competitive equilibrium with perfect knowledge.

There is little to add to the discussion of the index number problem. The movement of any price of quantity aggregate will be affected by choice of weight base so long as relative changes in prices and in quantities are correlated. Since the correlation is generally negative in the U.S. economy, recent price weights tend to produce a smaller in-

[19] Robinson, *op. cit.*, pp. 119-21.
[20] See John W. Kendrick, "Measurement of Real Product," in *A Critique of the U.S. Income and Product Accounts*, Vol. 22 of *Studies in Income and Wealth*.

crease in an aggregate of physical volume than earlier period weights. Since this is true of both capital stocks and output, ratios of the two variables are less affected by alternative weighting systems than is either variable alone. Nevertheless, it is desirable in comparing changes in real stocks between two points in time to use the prices of each as weights in order to bracket the difference in change.

Procedures used to cope with the various measurement problems are inevitably conventional, but if they are basically reasonable they result in meaningful measures. As Simon Kuznets has put it, "all concepts in the field of national income are, in one way or another, nonoperational. They are goals that forever elude measurement and for which measurable approximations are substituted. . . . All these operational measures assume meaning only because they are approximations to the 'purer,' non-operational concepts behind them."[21]

[21] Simon Kuznets, "Comment" on Edward F. Denison, *op. cit.*, p. 272.

RISK, THE DISCOUNT RATE, AND INVESTMENT DECISIONS

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The problem of risky investment decision has both normative and positive aspects. Looked at normatively, the question is: what is the appropriate technique of analysis—for individuals, firms, or government agencies—to use in evaluating risky investment alternatives? Looking at the problem positively, we ask: why do the mean experienced yields in different risk-classes of securities diverge, even when average yields for the different risk-classes are calculated over many individual securities and over long time-periods to eliminate random effects? If we first examine the positive aspect, the major explanations that have been offered for the yield divergences mentioned above (e.g., the divergences between stock and bond yields, or between earnings on industrial and utility shares) attribute them either to imperfections of capital markets or to market premiums for risk bearing. I will be adopting the latter approach. What I will be presenting is quite a simple market theory of risk, modeled upon Fisher's treatment of time preference and interest (but definitely not modeled upon Fisher's treatment of risk). On the normative side, I attempt to show how the criterion of maximizing present value—now generally accepted, despite some recent heresies, as the guiding principle for evaluation of riskless investment alternatives[1]—must be modified or generalized when risky investments are considered. In brief, I try to indicate the appropriate rate of discount to use, in the present-value formula, to allow for uncertainty of return as well as futurity of return.

I. *The Meaning of "Risk"*

First, however, it is absolutely necessary to clarify an elementary point that has been the source of much confusion. There are at least two quite different senses in which investments may be said to be risky. First of all, an investment with a certain nominal or quoted yield (e.g., a corporate bond) is often said to involve a risk of partial or total default. Here risk means only "unfavorable chance." Its measure is the difference between the nominal return and the true mathematical expectation of return. I shall call this difference or bias "expected-value

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risk," but the concept will not play any important role because I will
henceforth ordinarily be speaking of investment yield in the expected-
value sense. It is worth remarking, however, that expected-value risk
does not occur solely in dealing with investments of fixed nominal yield,
like bonds. It has been convincingly demonstrated, for example, that
the predicted benefit-cost yields of federal water-resource investment
projects are not, on the average, realized. Here also predicted yields are
biased estimates of the mathematical expectation of yields.

The concept of risk I will use herein, however, concerns not the
expected value but the variability of the probability distribution of out-
comes. For concreteness, we may think of the standard deviation, the
most common measure of variability, as a quantifier of "variability
risk"—or, as we shall say henceforth, simply risk. However, I do not
want to commit myself to saying that the standard deviation is *the*
measure of risk. It may well be that other moments of the distribution
are also involved in determining what we ordinarily call risk-premiums
on security yields.[2]

II. *A Market Theory of the Risk-Premium*

In this section I propose to present only some elements of a theory
of the market process by which risk-premiums on investments are de-
termined, in the belief that even a partial theory may serve to bring
some order to the subject and settle a few debated points. My basic
contention will be that the market risk-premium can be understood as
the interaction of individuals' willingness to bear variability risks and of
the technical fact of the productivity of risk, given individual endow-
ments of more and less risky income opportunities. I have quite de-
liberately stated these determinants of the risk-premium to suggest an
analogy with the theory of determination of interest—that is, of the
time-premium—that we associate primarily with the names of Böhm-
Bawerk and Fisher. In what follows I will attempt to develop various
aspects of this analogy.

First of all, we can distinguish between widening and deepening of
risk in a sense quite parallel to the familiar widening and deepening in
time. We widen investments in time when, given the period of the in-
vestment, we increase the aggregate of current sacrifice on behalf of
the future; we widen investments in risk when, given the degrees of
riskiness of differing securities (for concreteness, we may think in terms
of investments with specified standard deviations σ of yield per dollar
invested), we shift more of our current sacrifice from a relatively low-σ
to a relatively high-σ medium. We deepen investments in time when,

[2] Markowitz discusses the question of the appropriate measure of risk in his valuable
work, *Portfolio Selection* (Wiley, 1959), pp. 180-201, 287-97. See also James Tobin,
"Liquidity Preference as Behavior Towards Risk," *Rev. of Econ. Studies*, Feb., 1958.

holding the aggregate of current sacrifice constant, we shift from quick-yielding to slow-yielding investments; we deepen investments in risk when, given the amount of resources held out of secure media, we shift from lower to higher variability commitments of these resources. As a practical example of the distinction in terms of risk, consider a dry farmer in an arid region where returns are highly uncertain. For a particular set of lands subject to essentially identical conditions, he can widen his risk by cultivating more acres. Or he can deepen his risk by shifting all or part of his operations to lands with still higher variability of outcomes.

In the theory of risk, as in the theory of interest, it is simplier to analyze widening than deepening. This is especially so in the former case, as the standard deviation is a less perfect quantifier for depth of risk than its analogue—the period of production—is for depth in time. However, the main point is that, so long as we stick to the simple widening case, troubles about a perfect measure of depth of risk need not unduly concern us. Once we have made the vital distinction between widening and deepening, I believe it may be possible to grasp intuitively the general consequences of shifts in the depth dimension, even though a theory of the latter will have to wait for another occasion.

Limiting ourselves, then, to the pure widening case, we may start by asking ourselves what we can reasonably assert about the nature of individuals' preferences between, to take the simplest situation, only two investment media—on the assumption that in any case the investor sacrifices current consumption which is certain. One of these media may be taken to be a perfectly secure investment, with a known certain future yield. The other offers a known expected value of yield, subject to a specified type of variability. For concreteness, we may think of the unit of certain yield as, simply, a dollar—and of the unit of risky yield as a lottery ticket representing equal chances of winning $2.00 or nothing. It is necessary, to keep to the simplest possible situation, to assume that the returns from the risky units are perfectly correlated. In other words, if we have 1,000 lottery tickets, we must win either $2,000 or nothing. The correlation assumption prevents the intrusion of the law of large numbers. It is reasonably close to the true situation in our acreage cultivation example, where if the crop fails on any acre it is very likely to fail on all.

With this situation in mind, we can draw indifference curves, between certain yield and uncertain expected yield, that express individuals' risk-preferences. Indifference curve I in Figure 1 indicates that the individual in question is indifferent between $500 certain and an expected value of $1,000 representing equal chances of $2,000 or nothing. I feel very easy in asserting convexity of the indifference curves (di-

minishing marginal rate of substitution between certain income and expected uncertain income). Consider an individual whose initial endowment consists of a risky expected value of $1,000, with no sure income at all. It seems natural to assume that he would be willing initially to sacrifice expected value for certainty at a rate far better than 1:1—perhaps as high as 10:1. But as his certain wealth or income comes to attain a more balanced relation to his uncertain prospects, the rate of exchange he is prepared to offer for more certainty would become much more moderate. Indeed, it is conceivable that, given an

Expected Value
of Uncertain
Future Income
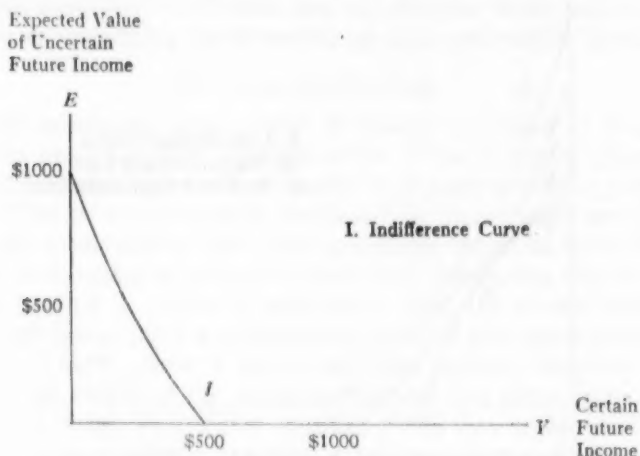
FIGURE 1. Indifference Curve Between Certain
and Uncertain Income

initial endowment consisting solely of $500 certain, the same individual might be willing to sacrifice more than $1.00 of certain income for an expected value of $1.00. If so, then in this range he would be demonstrating risk-preference rather than risk-aversion. However, for convexity it is not necessary to assert that the individual is ever a risk-preferrer (that is, that the slope of his indifference curve will ever be less than 1 in absolute value)—all that is required is that the rate of exchange he is willing to offer change steadily from one reflecting strong risk-aversion at the upper left to one representing much more moderate risk-aversion or even risk-preference at the lower right.

Having introduced the risk-preference function and the initial endowments, we may now bring in the productive opportunity locus and the market opportunity locus. In a "Robinson Crusoe" situation no market opportunities exist, and the individual must find his optimum in terms

of only his preferences, initial endowment, and productive opportunities. The essential point is the concavity of the productive opportunity locus, expressing diminishing marginal (expected) productivity of risk. The idea is that, by shifting his investments from the secure to the risky medium, the individual can first obtain a very favorable rate of exchange, but as he carries the process further it will become less and less favorable. The tangency slope will indicate the equality of the marginal rate of risk-aversion and the marginal productivity of risk-bearing.
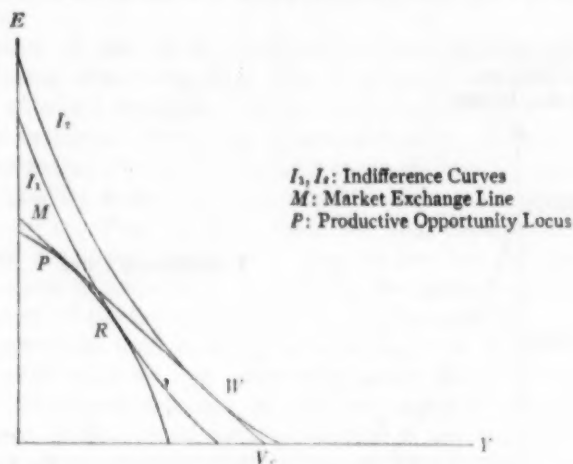
FIGURE 2. "Robinson Crusoe" Solution (R) and Market Solution (W) for Risky Investment

Finally, if we bring a market into our two-media world and assume perfect competition, there will be a governing rate of exchange between certain income and expected income, which will be determined in such a way as to equate the sums of individuals' desired holdings in the two media with the actual quantities socially available in terms of original endowments and productive transformations between the two forms. As in the familiar Fisherian solution for time-preference decisions, the insertion of the market exchange line permits attainment of a higher indifference curve than would otherwise be possible. In Figure 2 the "Robinson Crusoe" solution is at R, while the market for risk-bearing permits attainment of the point W. With the introduction of the market, we may speak of the individual's decision criterion as maximization of certainty-equivalent value (V_e in the diagram), in analogy with the maximization of present value in the theory of time-preference.

I do not feel it necessary to describe how these indifference curves

can be made consistent with Friedman-Savage utility-of-wealth functions based upon the von Neumann-Morgenstern postulates. The indifference curves, to use a horrible metaphor, stand on their own feet as descriptive of what seems to me obviously reasonable and typical patterns of behavior. Other analysts have not, so far as I am aware, arrived at the simple results outlined above. I believe the reason is that they unconsciously started with the more difficult deepening case— leading most frequently to the construction of indifference curves between expected value of return and standard deviation of return.[3] Unfortunately, assertions about the shape of these indifference curves carry little immediate conviction, and the situation is not improved by appeal to an underlying Friedman-Savage utility-of-wealth function.

III. *Some Implications*

Without attempting strict proof, I believe a number of refutable statements can be derived as implications of the foregoing approach. First and foremost, there will ordinarily be a positive market premium on risk. That is, expected risky yields will be higher than sure yields. This is due to the general tilt of the preference functions illustrated in Figure 1 (expressing reluctance to bear risk) interacting with the fact that risk-bearing is ordinarily productive. That the market does pay positive risk-premiums is a statement so refutable that many people (in particular, Knight) claim it has in fact been refuted. However, I believe that the weight of the evidence indicates that risky media of investment do in fact have higher expected yields than secure media (or, in other words, uncertain expected values are discounted relative to certain values). Over any reasonably long period of measurement, stocks yield more than bonds, and risky bonds more than secure bonds.[4] Of course, this theory does not allow for acceptance of "unfair" gambles. Alternative explanations would have to be sought for the phenomenon of Las Vegas.

A second implication is that individuals will typically diversify. The relationships among the curves are such that interior optima are normal, and corner optima exceptional. A third implication is that—so long as we are dealing with interior rather than corner optima and the assumption of perfect markets applies—on the margin everyone has the same degree of risk-aversion. We should, therefore, exercise care before labeling some individuals "risk-preferrers" and others "risk-avoiders."

[3] See Tobin, *op. cit.*, pp. 71-82; F. A. and V. Lutz, *The Theory of Investment of the Firm*, pp. 179-92.
[4] That stocks yield more in the long run than bonds is a notorious fact not requiring demonstration here. The recent study by W. Braddock Hickman, *Corporate Bond Quality and Investor Experience*, supports my contention about bonds—that investors demand an extra premium, over and above the expected loss by default, to hold risky bonds.

What is true is that, in the process of attaining marginal equality, some individuals will have shifted away from relatively secure initial endowments and so in a sense sought out risk, while others will have moved in the opposite direction; but this shift will reflect not merely preferences as to risk-bearing but also initial endowments and access to risky productive opportunities.

Translating the third implication above into normative language, we may say that all investors should discount risks at the rate of exchange between certain and risky prospects established by the market for the risk-class within which the investment in question falls. This contradicts a traditional view that, in evaluating risky investment opportunities, the investor ought to specify the expected values involved and then discount them by some kind of "caution coefficient" expressing his own preferences toward risk-bearing.[5] The error here has an analogy in the domain of time preference, where theorists have sometimes advised investors to use their personal time-discount rates rather than the market rate in discounting future income prospects. In the latter case, it is not difficult to show that, given a perfect market for exchange of present and future funds, highest levels of satisfaction are attained by maximizing present value calculated at the market rate of discount. Similarly in the case of risk, the optimization rule is to maximize certainty-equivalent value, discounting uncertain expected yields at the market rate of exchange between such yields and certain ones (always assuming that perfect markets exist). In the one case, this implies adjustment of all personal marginal time preferences, by appropriate shifts between present and future funds, to the market time-discount rate; in the other, adjustment of personal risk-preferences, by appropriate shifts between secure and risky media, to the market risk-premium rate.

IV. *Corporate Finance and Investment Decisions*

The analysis presented above suffices to provide the answer to the simplest problem of corporate finance. If the investment in question involves only widening in time and in risk (i.e., shifting from secure and current funds to future and uncertain ones of a given degree of risk), if it is a new venture so that there are no interactions with returns on past investments, and if only equity financing is involved, the market time-and-risk discount rate should be used in deciding how far to carry the investment. While the list of simpifying conditions sounds formidable, I should comment that the market discount rate in question —the "impure" (inclusive of risk-premium) rate of interest—is at least more of an observable magnitude than the abstract riskless rate of

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[5] Irving Fisher, *The Nature of Capital and Income*, p. 277.

interest. It is assumed that the entrepreneur and the potential investors are both aware of the mathematical expectations of returns at different dates and the appropriate market capitalization rate, $1/\rho$ (where ρ is the "impure" interest rate) for expected yields in that risk-class. The investment in question should, of course, be widened until its marginal expected yield per current certain dollar sacrificed falls to ρ.

The listener may possibly have detected by this time a certain resemblance to the line of thought put forward in the recent provocative article by Modigliani and Miller.[6] Modigliani and Miller go beyond the analysis presented here in centering attention upon the market yields of debt-equity combinations and the implications thereof for investment decisions. However, I believe that the theoretical analysis of risk-bearing presented here is that implicitly underlying the Modigliani-Miller paper. The most essential parallel is that Modigliani and Miller select market-value maximization as their criterion (page 264), which in the context is essentially maximization of the certainty-equivalent value described above. The Modigliani-Miller "equivalent-return classes" (pages 266ff.) seem to be essentially what I have called "risk-classes"; in addition, Modigliani and Miller assume a positive market premium for risk bearing (page 271).

Limitations of time, unfortunately, prevent my presenting a fuller analysis here of what I regard as the major normative conclusion of the Modigliani-Miller analysis: that, setting aside consideration of corporate income tax, even firms with complex debt-equity capital structures should use, in evaluating investment alternatives, the discount rate determined by the market in capitalizing pure equity streams of comparable risk (page 288). I believe that this conclusion is basically sound. It is, with some qualifications, quite consistent with the theory of risk bearing presented above, provided one accepts in addition the famous "Proposition I" of Modigliani and Miller: that the "value of the firm," the sum of the market value of its debt and equity, is a constant—determined by capitalizing its expected asset return at the appropriate risky discount rate (page 268). Without getting into tortured debates about what constitutes "arbitrage," I would agree that a divergence between a firm's net asset value and the sum of its debt and equity cannot be expected to persist—unless market imperfections are brought in.

There is one qualification I would like to make here, however, arising out of the theory presented above. Modigliani and Miller describe the discount rate recommended for investment decision as the pure-equity capitalization rate for the asset-earnings of the firm as a whole. Their

[6] Franco Modigliani and Merton H. Miller, "The Cost of Capital, Corporation Finance, and the Theory of Investment," *A.E.R.*, June, 1958, pp. 261-97.

risk-class concept is a characteristic of the firm rather than of the individual investment. Evidently they are thinking of marginal investments as being equally risky with those previously adopted—what I have called risk-widening rather than risk-deepening. This is justifiable as simplifying the analysis, though the problem remains of evaluating risk-deepening investments. Another problem is created by imperfect correlation of investment returns. While the theoretical analysis above assumed perfect correlation to avoid the operation of the law of large numbers, in the real world the advantages of risk-pooling are very important. In fact, a marginal investment that is individually very risky may substantially reduce the over-all variability risk of the firm's total portfolio of investments. I am inclined to think that the best way of rescuing the analysis is to take note of the fact that, where large numbers of individual investments are involved, the variability of the over-all investment return approaches the value of the average covariance among the individual investments.[7] The risk-classes into which firms are divided, then, would depend very importantly upon the diversity of returns of their portfolios as a whole, and the variability of individual investments can be correspondingly neglected. The established all-equity discount rate for the firm can then be used without great error in evaluating marginal investments whose individual variability and intercorrelation with other investments are not too divergent from the over-all pattern.

[7] See Markowitz, *op. cit.*, p. 111.

DISCUSSION

FRANCIS M. BATOR: Professor Eckstein is perhaps a little too hard on capital theory. Admittedly, the theory is weak in its empirical underpinnings and its formal structure. Nonetheless, I am inclined to be a little more sanguine than he about its uses in development planning. This could be, partly, because my expectations about the potency of pure theory may be more modest than his, and partly because I am more impressed than he appears to be by the importance of some of the insights which emerge from the theory; e.g., the tentative, partial, other-things-equal "rules" regarding choice among, and design of projects, rules which are implied by simple-minded small models based on crude assumptions about technology, factor scarcity, and so forth.

To be sure, most of the insights one can derive from capital theory—insights of the sort Eckstein mentions in the last section of his paper—seem rather obvious, once you see them. But as an empirical matter, it appears to be rather easy for practical men not to see them. And they are ignored at great cost.

Still, it is true, capital theory is a meager foundation for a full-fledged normative theory of economic development. Indeed, even if capital theory were entirely satisfactory on its own terms, in presenting us with the implications of intertemporal Pareto efficiency in production—of maximization subject only to given endowments and the current technology—even if it were able to do that, much of importance to the planner would be left out: direct interaction between allocation, tastes, and technology; a lot of "noneconomic" organizational values; non-probabilistic uncertainty. And, of course, we are far from having solved even that limited problem. We know pitifully little about production functions, even about existing practice (especially its time structure), and in choosing our hypothetical functions we are governed more by what makes the mathematics come out right (and, for that matter, the economics) than by any conviction that, for instance, the world is convex. If I am less bearish than Eckstein about the usefulness of capital theory to the development planner, I am less bearish only in degree.

However, it may be that we are bearish for somewhat different reasons. Even if the theory were flawless, I would not expect it to yield much of an answer to the hoary question of the optimal rate of saving. Indeed, one great merit of the theory is that it makes clear the need for stronger value judgments, additional to the principle of consumer sovereignty. Of course, Eckstein is well aware of the need for a multiperiod welfare function. What is not clear to me from his paper is why this need and the fact that capital theory as such cannot satisfy it should be scored as a failure of the theory.

There is a further, less formalistic reason why I am not much concerned, given our topic, about not being able to give the planner a philosophically satisfying answer regarding the optimal rate of saving. One suspects that in a low-income country like India, the planner finds it only too easy to impose

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enough additional constraints—additional to resources and technology—to leave him little leeway. It is not that he will have no choice about the aggregate rate of saving, but that crude political considerations having to do with an "acceptable" minimum rate of growth and an "acceptable" and feasible maximum of forced saving and of coercion, are likely to determine the answer. (Matters are, of course, different when it comes to deciding about the rate of aggregate saving and investment in the United States.)

What about the second reason suggested by Eckstein for failing capital theory: that it does not provide a reliable, quantitative relationship between the rate of aggregate investment and the rate of growth? This is an important test, but is it not too difficult? Even a near-perfect capital theory would not be likely to yield an aggregate production function which gives really good predictions, certainly not a simple function of flow-variables. We resort to the crude empiricism involved in much macroeconomics because microeconomics, based on physical constraints and behavior relations, is so very hard. But it is perhaps not quite fair to judge the quality of our microtheory by whether or not its results can be aggregated into simple, stable macrorelationships which yield good predictions.

What then are the tests to apply? I am inclined to think that it is by its success or failure to provide criteria for the efficient allocation of investable resources, so called, that normative capital theory should be judged. That is a test both important and appropriate to the pretensions of the theory.

That it is appropriate is plain from the literature of capital theory. To be sure, until recently economic theorists have concerned themselves with the problem of investment criteria primarily in its partial equilibrium aspects— with the decision rules followed, or to be followed, by the individual firm. However, intertemporal general equilibrium has also received attention. In fact, it has been shown that under the usual neoclassical assumptions, decentralized present-value maximization by competitive producers of limited but accurate foresight operating in a stationary world will sustain an intertemporal allocation of investment that is Pareto-efficient.

Perhaps inevitably, however—and, from the point of view of the strategy of theorizing, sensibly—general-equilibrium analysis of the efficiency of investment decisions has proceeded under assumptions about convexity, certainty, etc., most likely to yield satisfying theorems akin to those implied by the statical Walrasian flow-model. As a result, we have developed criteria for allocation that are efficient in precisely the circumstances which almost void the need for such criteria—circumstances under which a more or less idealized price-market system, in either its capitalist or its Lange-Lerner form, can be relied on to do a more or less efficient job. In contrast, we remain quite in the dark about criteria for efficient allocation as regards activities characterized by substantial lumpiness and increasing returns to scale—activities relative to which even error-free profitability calculation will do badly by the community and which, therefore, call for alternative procedures of economizing calculation and organization. (I slur over the difficult "second-best" problem.)

Since by the narrow, parochial but important standard of efficient alloca-

tio⋯ ⋯⋯ *raison d'être* of the central planner is to provide substitutes for decentralized market-type criteria where these would work badly, the lack of good criteria for other than a situation where idealized markets are efficient is certainly critical. The more so in a low-income context, where the quantitative significance of economies of scale (compounded by interindustry complementarities, transport cost, and imperfections in international trade) is likely to be greater than in advanced countries, and where, unlike in a high-income country, a little more or a little less efficient use of resources can make the difference between stagnation and growth.

I emphasize nonconvexity, not because I think it the central difficulty in achieving tolerably efficient allocation in even a relatively well-organized low-income country. But we can hardly expect economic theory—or, for that matter, central planning—to help with civil disorder, or cultural disinterest in economizing, or a shortage of managerial talent, or with grand-dynamical interactions involving tastes and motivations. One can even excuse the failure of the theory seriously to deal with uncertainty, though that is certainly central to the problem of capital. But our ignorance, in the face of substantial nonconvexity, about the implications of maximization even in an exact and stationary context is a different matter. That is a failure of the theory on its own terms. The problem does not even involve, in an essential way, stationary dynamics. But it is compounded by the dynamical fact of capital. Indivisibility through time (durability), which does not give rise to nonconvexity, and indivisibility in scale (lumpiness), which does, appear to be correlated. And durability makes mistakes due to lumpiness more costly, less reversible. Moreover, some of the most bulky, durable facilities—the so-called "social overhead" facilities—generate services that are, speaking loosely, both indispensable, even at a very low income level (i.e., are part of any efficient package), and also not importable. Transport cost in many important instances is virtually infinite.

To assert a need for rules of allocation which will do well where private market calculation does badly is easy. To do something about it, less so. But there are some things that can be done which do not require a major breakthrough in pure theory. For one thing, it is possible to identify with greater precision and generality than has been done heretofore the causes of market failure, the areas where market mediation is likely to do badly. Such identification also serves to reveal where market calculation is apt to do well. Given the political bias in the low-income countries to reduce rather than expand the area of market mediation, explicit identification of sectors where markets are likely to be efficient is needed to offset the tendency to underuse the price system. This is important because the price system is likely to be much better than more centralized procedures at avoiding "local inefficiency." Not to use markets where they do well "in the large" is as foolish, if one is concerned to economize, as to try to use them where they do very badly (though this formulation is somewhat misleading, in that it begs all the difficult problems due to the all-or-nothing quality of market efficiency).

It is possible, also, to do much more than has been done so far to explore

some of the qualitative implications of increasing returns-to-scale for efficient allocation. By means of very simple models, which make no attempt at realism, it is possible to gain considerable insight about the qualitative patterns which are consistent with maximizing and about the efficiency of various decentralizing rules.

And again, it is feasible and useful to test the great variety of explicit and implicit rules of thumb which are to be found in the technical and planning literature, the journal articles, and the five-year plans against more generally valid rules of maximization; i.e., to explore under what assumptions there is a correspondence. This should help, not only to separate what is sensible and useful from all that is not, but to establish the "range of validity," as it were, of the rules that do make sense.

Also, there is great progress to be made in developing computational procedures for nonconvex programs. Good algorithms can yield qualitative insight as well as quantitative answers.

Finally, it is possible to use modern theory in the design of a coherent planning strategy—a strategy involving judicious use of quantitative planning of allocation; qualitative, rule-of-thumb type checks on such planning; and "planning" in terms of institutions.

VERNON L. SMITH: In the laissez faire spirit that a discussant ought not to be required to discuss, I propose to use my time for the purpose of suggesting a theory of investment decisions under risk. In achieving this I shall build directly upon a selection of ideas which emerged from reading Mr. Hirshleifer's paper. Any disagreements I might have with his paper will be evident in my argument.

The intriguing idea occurring to me in reading this paper is the possibility of developing a theory of investment decisions which simultaneously accounts for time preference and risk. It has long been evident to me that all investments have a twofold characteristic: They require certain present wealth to be sacrificed in order to obtain future wealth or income and th-future wealth so obtained is uncertain. Rarely can an investment be so perfectly hedged that it can be analyzed entirely in terms of time preference. Similarly, in long-term investment decisions one cannot ignore time preference and consider only the risk dimension. Time and risk are joint attributes of the typical investment.

In this spirit, consider the ordinal utility hypothesis $U = U(Y, M, p)$, where Y is certain present wealth or liquidity, and M is the uncertain additional wealth to be had at some given future date, as a consequence of buying M shares of a security with probability p of paying \$1.00 each at that future date, and probability $(1-p)$ of paying nothing. [In this example expected future wealth from investment is $Y + pM$ with variance $p(1-p)M^2$, but these are irrelevant facts.] This utility function abstracts only from deepening in time, which is easily fixed by writing $U = U(Y, M, p, T)$ where T is the investment period. But let us keep to the simpler case. Observe that I have substituted present certain wealth for Hirshleifer's future certain income, and uncertain future wealth for his expected uncertain future inl

come. Only the first substitution represents a substantive change. A norma-indifference surface corresponding to $U(Y, M, p)$ might be as shown in the two-dimensional mapping of Figure 1 for different probability and satisfaction levels. Points $A-D$ corresponding to different (Y, M, p) combinations

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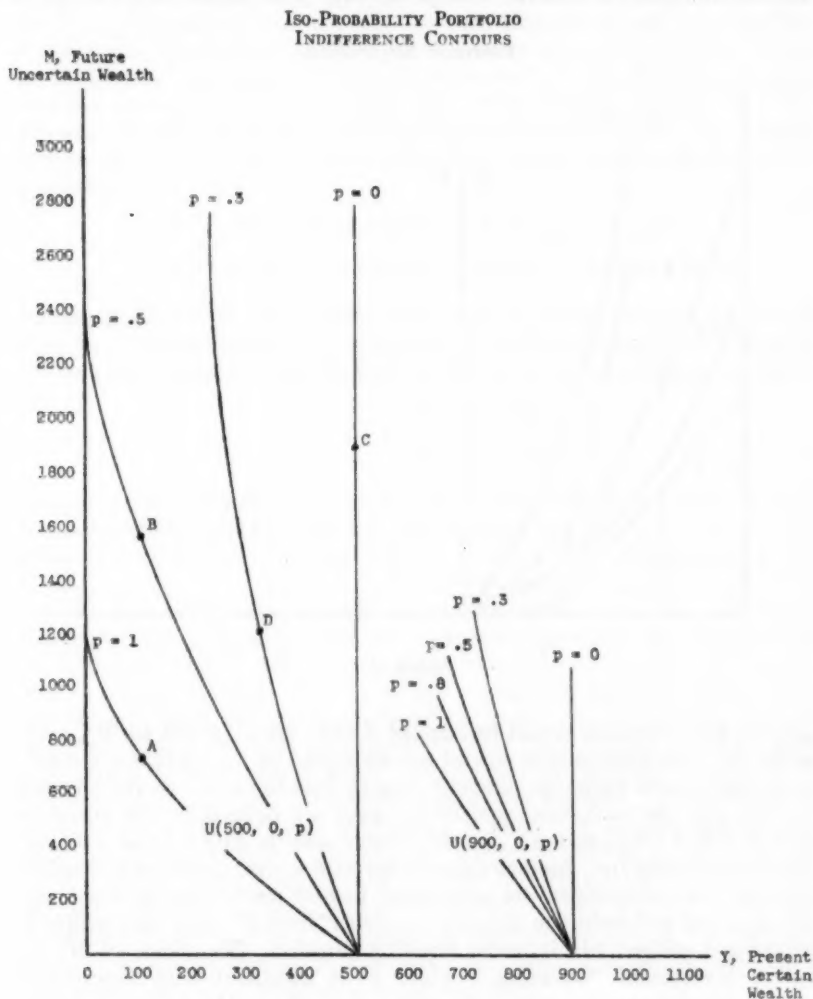
FIGURE 1

are all indifferent at the utility level $U(500, 0, p)$, $0 \leq p \leq 1$. Note that this utility hypothesis becomes Fisher's theory when $p=1$, and Hirshleifer's (except for the above qualifications) when $p=1/2$. As illustrations of the

application of this hypothesis, I shall consider two kinds of decision problems. In both cases let me simplify the problem by considering only exchange opportunities. In the first case suppose the individual faces a continuum of investment opportunities defined over the unit probability interval. The price of a share with probability p of pay-off is $1/1+i(p)$. A

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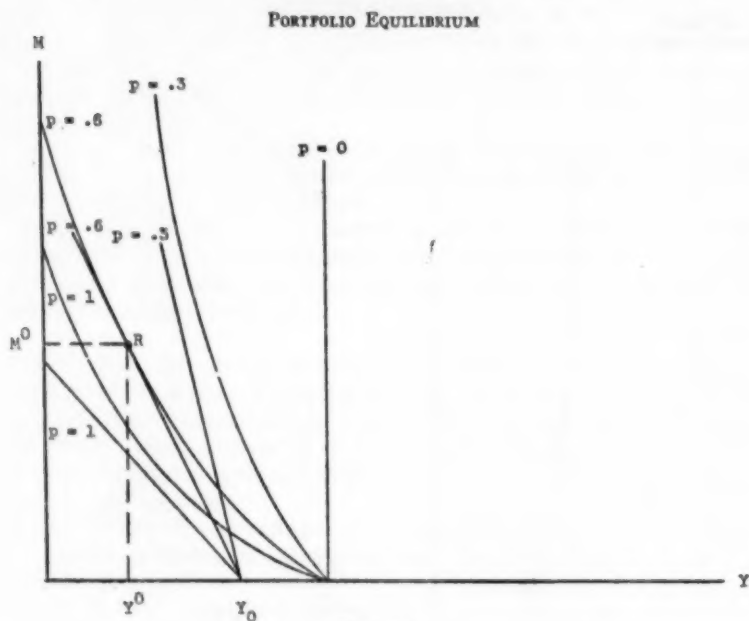
FIGURE 2

positive risk premium would be implied if $i'(p)<0$, $i''(p)>0$, for $0\leq p<1$, while the pure riskless time cost of money would be $i(1)$. This price represents the ratio of exchange between present liquidity and uncertain future wealth, and the exchange opportunity lines are defined by the equation $Y_0=Y+M/1+i(p)$, where Y_0 is initial liquid wealth. At $p=1$ this becomes Fisher's exchange line. Assume that our investor desires to choose a liquidity level, Y^0, an uncertain future investment pay-off level (number of shares), M^0, and one and only one security (i.e., "risk" level, p^0) such that utility is maximized subject to his initial liquidity position. The choice of M^0 involves investment "widening," while p^0 is identified with investment "deepening," if one wishes to make such a distinction. The decision margins of liquidity, investment, and "risk" are given by the conditions $\partial U/\partial Y-\lambda\leq0$, $\partial U/\partial M-\lambda/1+i(p)\leq0$, and $\partial U/\partial p+\lambda i'(p)M/[1+i(p)]^2\leq0$, where λ is the Lagrangian marginal utility of present liquid wealth. An interior maximum is implied when the equalities hold, and is illustrated by the point R in

Figure 2, while the inequalities generate boundary solutions with, e.g., all wealth held as cash, or as shares.

The above case does great violence to the facts by permitting the investor to diversify as between only one risk class of security and cash, but it has the merit of casting the concept of "risk" margin or "deepening" (if this concept is useful) in bold relief. Let me relax this restriction by considering a simple capital market in which only two securities are offered. Security No. 1 has a fixed pay-off probability p_1^* and price $1/1+i_1$, while No. 2 has probability p_2^* and price $1/1+i_2$, where $i_1 = i(p_1^*)$, $i_2 = i(p_2^*)$. Under a positive risk premium $i_1 > i_2$ if $p_1^* < p_2^*$. The utility function is now $U = U(Y, M_1, M_2, p_1^*, p_2^*)$, where M_1 is shares held in security No. 1 and M_2 is shares of security No. 2. Under the expected utility axioms this function could be written

$$U = U(Y, M_1 + M_2)p_1^*p_2^* + U(Y, M_1)p_1^*(1 - p_2^*)$$
$$+ U(Y, M_2)(1 - p_1^*)p_2^* + U(Y)(1 - p_1^*)(1 - p_2^*),$$

but we shall have no need to restrict the analysis in this manner. We assume that the investor chooses a liquidity level, Y, and investment levels, M_1 and M_2, so as to maximize utility subject to the exchange opportunity equation

$$Y_0 = Y + \frac{M_1}{1 + i_1} + \frac{M_2}{1 + i_2}.$$

Note that the terms $M_j/1+i_j$ are like the discounted present values of riskless investment theory except that the discount rates i_1 and i_2 include risk premiums as well as the time cost of money. [The risk premiums are $i_1 - i(1)$ and $i_2 - i(1)$.] $M_j/1+i_j$ is the present certain equivalent value of a sum M_j, due with probability p_j^*, at some specified future date. The marginal conditions on liquidity, and the investment levels in each security can be written $\partial U/\partial Y - \lambda \leq 0$, $\partial U/\partial M_1 - \lambda/1+i_1 \leq 0$, and $\partial U/\partial M_2 - \lambda/1+i_2 \leq 0$. For an interior solution these conditions require the marginal utility of the last dollar's worth of liquidity to equal the marginal utility of the last discounted dollar's worth of investment in each risky alternative. Boundary solutions are entirely likely with all wealth held in the form of cash and/or either security. For smooth interior maxima we can think of solving these equilibrium conditions simultaneously with the exchange opportunity equation to obtain the individual's asset demand functions for cash, and for each security. These demand functions will be of the form $Y = G(Y_0, i_1, i_2, p_1^*, p_2^*)$, $M_1 = H_1(Y_0, i_1, i_2, p_1^*, p_2^*)$, and $M_2 = H_2(Y_0, i_1, i_2, p_1^*, p_2^*)$. The demand for cash and securities depends upon initial wealth, the discount rates on securities, and their "riskiness." A discussion of the implications of this theory will have to await another occasion.

ZVI GRILICHES: The problems associated with attempts to measure something called "capital" can be divided, I think, into two groups. About a third of these represent particularly virulent examples of garden variety index num-

ber problems. The other two-thirds are basically specification problems—problems of defining what type of measure should be used for what purpose. It is this last type of problem that is glossed over in Professor Kendrick's admirable discussion of capital measurement by his concentration on a search for *the* capital measure. Progress in this area, I believe, depends on the explicit recognition that different measures will be useful for different purposes and that no one of them will do for all.

Perhaps the most serious problem that arises in trying to measure output, input, or price changes is the one caused by the changing nature of the items being measured or priced—the quality change problem. Consider the introduction of a new version of a machine in period one at a price of 105. The old version sold at a price of 100 in period zero (and I am abstracting from general price level changes). Assuming competitive market conditions Kendrick would measure this new machine as "5 per cent more machine than the old version." This is a very reasonable measure as it tells us how much of resources was "given up" in producing this particular machine. We may, however, also know that the buyers of these machines would have been willing to pay 110 for each one of the new machines if it had been available in period zero.[1] They may have been willing to do so because from the point of view of the production function a unit of the new machine equals 1.1 units of the old ones. If we are interested in measuring the shifts in the production function of the widget industry, the buyer and user of these machines, we would count the new machine as "10 per cent more machine" and would indicate a drop in machinery prices.

The disagreement between us is to some extent a semantic one but my approach would put a premium on measuring something interesting that may not get measured otherwise; i.e., quality change. I would expect that the productivity or value to purchaser based capital measure would prove at least as interesting a concept as the real cost one. On the other hand, a consistent application of the real cost concept may raise more problems than it will solve. It would require that purchases of machinery should not be deflated by a machinery price index to get at a constant price (or rather constant cost) capital measure but by some more general price index for the economy as a whole, since all relative machinery price changes are presumably a reflection of changes in real costs.

Another type of problem arises if we ask whether we should accept the used machinery market evidence on depreciation patterns and rates in constructing our capital estimates. We surely should if we are interested in getting an estimate of the current value of the existing stock of capital; but this may not be desirable if we are interested in estimating the quantity of capital in constant prices. Part of the observed fall in the value of machines with age is not due to any deterioration in their capacity to provide services but to the

[1] How one would know this is a separate, difficult, but in principle not insoluble problem. For an attempt at measuring something like this, see my "Hedonic Price Indexes for Automobiles: An Econometric Analysis of Quality Change," NBER-Bureau of the Budget Price Statistics Review Committee Staff Report No. 3.

appearance of new and better machines at lower real prices to obsolescence. But this drop in value is really a price change. This price change happens because new machines are better, not because old ones are worse. As long as the old machines keep performing the same old tasks, we should not write them down in our measure of capital as a productive input, even though their relative price and the value of their services (but not their quantity) have declined. Retirements are a legitimate deduction from this type of capital measure but obsolescence is not. To the extent that observed depreciation rates are affected by obsolescence—an undoubtedly important factor—these rates may be inappropriate for use in productivity oriented capital measures.

The very important distinction between the stock of capital and the flow of services derived from this stock is not made clear enough in Kendrick's paper. Even if the rate of capital use were to remain constant, the two measures may move differently over time if there are changes in the age distribution of capital. This would happen because the stock of capital is the value (in some constant prices) of present and all future services. But for many purposes—in particular for productivity analysis—we may be interested mainly in the current flow of services from this stock. The usual type of capital measure may reflect very little of this. Moreover, we are likely to be interested in the amount of services used rather than the amount available. Thus it would surely prove interesting here also to distinguish between labor force and employment type concepts. It may be difficult to measure, but we should not overlook capital unemployment.

In the short run, the stock and flow measures may diverge sharply if there is a change in the rate at which the given capital stock is being utilized. Thus if, for example, the widget industry had changed from a one- to a two-shift per day schedule, the usual type of productivity measures would show a large increase in the output to capital ratio. But in fact there has been no real change in the production function; all that we are observing is the input of more machine (and man) hours and an increase in output. The production function coefficients relating capital services to output have remained unchanged though the profitability of using capital in the widget industry may have changed. The basic problem here is that the usual productivity measures confuse movements along given cost curves (changes in profitability) with shifts in the curves themselves (technical change proper). We need different measures of capital and of productivity to be able to distinguish between these two very different kinds of phonomena.

To summarize, I would argue that we may need at least four different measures of capital: (1) A measure of the value of capital stock in constant general prices, as a measure of the real wealth of firms which may affect many aspects of their behavior. Such a measure would be net of obsolescence. (2) A measure of the physical quantity of capital; that is, the number of machines weighted by their relative equilibrium prices. For this measure, observed depreciation rates may not be relevant if obsolescence and hence disequilibrium play a large role. (3) A measure of available services, measured by available machine hours weighted by their respective equilibrium rents per

machine hour (in constant prices). This weighting pattern may differ from the previous one since the yearly rent for a 99 year old one-horse shay will be the same as for a 1 year old one, though their market prices will differ greatly. And (4), the quantity of services actually used. It is this last concept that is of relevance when we try to measure the rate of technological advance. In the case of all of these measures, I would try to adjust for quality change to the best of my ability.

Whether or not these four measures are right or enough, it is clear to me that one capital measure will not do. Even if there is a series labeled "K" on page 497 of the second volume of Professor X's treatise, it is improbable that it is just what we need. We are likely to learn more and create less havoc if we concentrate on defining and measuring that concept of capital which is most relevant in answering the particular question that we are interested in. There is no panacea.

MANAGERIAL ECONOMICS: A NEW FRONTIER?

THE CURRENT STATE OF MANAGERIAL ECONOMICS

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I. *Introduction*

In a book[1] whose title is imprinted on this session, Joel Dean opens
with the following statement: "The purpose of this book [on man-
agerial economics] is to show how economic analysis can be used in
formulating business policies." Some eight years later, in the preface to
a book[2] whose subtitle is, "An introduction to managerial economics
under uncertainty," R. Schlaifer writes: "In the author's experience,
thoughtful businessmen intuitively apply this kind of analysis [i.e., the
kinds of analysis which appear in modern versions of subjective prob-
ability and statistical decision theory] in problems which are simple
enough to allow of purely intuitive analysis; and he [i.e., Schlaifer]
believes that they will readily accept its formalization once the essential
logic of this formalization is presented in a way that *can* be compre-
hended by an intelligent layman."

These remarks suggest that, in managerial economics, it is the view-
point of (business) management which is decisive either in the kinds
of problems that are addressed or in the modes of reasoning which are
admissible. Both authors would probably agree to certain qualifications
and extensions that would be needed to provide a reasonably compre-
hensive view of managerial economics in all of its current manifesta-
tions. Even if the primacy of practicing management were admitted in,
for example, the choice of problem material, the existing state of scien-
tific knowledge—in managerial economics and elsewhere—would have
to be brought into consideration in order to assess the likely promise of
research accomplishments. Moreover, the research and teaching of this
topic is—or should be—conducted by persons who are steeped in the
traditions of scientific research and it might be expected that this would
be reflected in their choice of problems, modes of analysis, etc. In par-
ticular, it might be expected that they would have their attention di-

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[1] Joel Dean, *Managerial Economics* (Prentice-Hall, 1951). See also footnote 19, infra,
and the associated discussion for certain necessary qualifications.

[2] R. Schlaifer, *Probability and Statistics for Business Decisions* (McGraw-Hill Book Co.,
1959).

rected by the activities in science—e.g., towards exploiting possible scientific breakthroughs—even when this does not necessarily lead to managerial applications, or even when this occasions a wide departure from a balanced research approach such as, say, a managerial consensus might conceive to be appropriate.

The following examples,[3] originating in work done by managerial economists, is perhaps sufficiently illustrative: (1) research designed to establish interconnections between outstanding disciplines such as queuing theory, statistical decision theory and linear programming in order to deepen our understanding of each and to provide a basis for further generalization;[4] (2) a simulation study—or more precisely a raw (or unguided) simulation—whose objective is to achieve an understanding of the empirical facts concerning the behavior of firms in a certain industry, as judged by the pertinence of classifications which can be used to identify the behavior patterns which are generated and the predictive power which these classifications provide; (3) a guided simulation study wherein an analytical model and an associated optimizing principle are used to eliminate irrelevancies which might otherwise creep into the simulations and provide the wanted predictions—and analytical characterizations—in a way that can be more readily understood.[5] So far as I can ascertain, these studies do not differ in any way from either the approaches (or motivation) that might be used in any part of economics.

These examples are, it may be objected, drawn from the literature of operations research and management science. But the advent of these (and related) activities has provided a flux which has pervaded all parts of managerial economics. Since it is also likely to affect other parts of economics, it is not wise to omit these developments—by reference to a difference in labels—in any attempt to assess the current state of managerial economics. In much of the remaining portion of this paper I shall therefore be concerned with operations research and like activities, which I shall explore for their possible bearings on developments in economic analysis.

[3] There are numerous examples from which illustrations could be drawn but I here restrict myself to citations from my own work simply because I can thereby be more certain of the problem context and the underlying motivation.

[4] A. Charnes and W. W. Cooper, "Chance-Constrained Programming," *Management Science*, Oct., 1959.

[5] A. Charnes and W. W. Cooper, "Extremal Principles for Simulating Traffic Flow in a Network of City Streets," *Proceedings of the National Academy of Sciences*, Feb., 1958, pp. 201-04. It should perhaps be emphasized that the optimizing objective—extremal principle—is utilized in a manner wholly akin to, say, principles of "least action," etc., in the natural sciences, or to the way profit maximizing or minimizing objectives are sometimes said to be used in economics—e.g., to provide a simple way of analyzing substitutions that might be affected under varying prices. (For further discussion see Chap. XX in the reference cited in footnote 8, infra.)

II. *Operations Research and Managerial Economics*[a]

It should occasion no surprise that scientifically trained and oriented persons, when engaged in management science research or operations research applications, should remain mindful of opportunities to contribute to the body of scientific knowledge. The fact that numerous economists (including speakers on this panel) have participated in these developments should then carry the obvious corollary of potential contributions to the body of economic knowledge and methodology. But, of course, this might also be true if this same personnel were engaged in almost any other conglomerate of activities and there is therefore a need to set forth relations between, say, operations research and economics in somewhat more specific terms.

It is not possible here to undertake the forbidding task of surveying the instant fields of operations research, management science, etc., whose literature already runs into thousands of citations.[b] I shall therefore take a different tack and distinguish between three areas of management activity: planning, operations, and control.[c] The planning phase of management with its *ex ante* emphasis and specification of alternatives prior to effecting a designated course of action is most clearly related to the received traditions of economic analysis. The area of "operations," which is concerned with decisions actually to commit resources, and the area of "control," which is concerned with conformance between plans and operations, are somewhat more removed from these traditions.

Doubtless the latter areas will subsequently become more central in management science and operations research. To date, however, it is the area of planning that has received the greatest attention. More specifically, the main concern has been with the mechanisms of rational choice in "functional" as distinct from "organizational" planning. That is, the focus has been on rational modes for effecting inventory, price, production, and distribution decisions rather than on the planning of

[a] The excellent and recent article by R. Dorfman, "Operations Research," *A. E. R.*, Sept., 1960, makes it unnecessary for me to provide a detailed description or definition of these terms. See also "Economics and Operations Research: A Symposium," *Rev. of Econ. and Statis.*, Aug., 1958.

[b] Cf., e.g., Case Institute of Technology—Operations Research Society of America, *A Comprehensive Bibliography on Operations Research* (John Wiley and Sons, 1958). This volume, which contains some 3,000 citations, is already dated, and hence inadequate and would therefore need to be supplemented in a variety of ways; e.g., by reference to more specialized bibliographies such as V. Riley and S. Gass, *Linear Programming and Associated Techniques* (Johns Hopkins Press, 1958) and D. M. and G. L. Thompson, "A Bibliography of Game Theory," in A. W. Tucker and R. D. Luce, eds., *Contributions to the Theory of Games*, IV, Ann. Math. Studies No. 40 (Princeton Univ. Press, 1959).

[c] See A. Charnes and W. W. Cooper, *Management Models and Industrial Applications of Linear Programming* (John Wiley and Sons, 1961) for more detailed discussions of these concepts.

organizing and staffing relations of the kind which have been the primary concern of organization theorists.[9]

To date, then, the center of attention in operations research is one which is indistinguishable from the kind of orientation that an economist—even a fastidious one—might be expected to supply. As movement is effected towards the control and organization aspects of management problems, a rather heavier draft on the other behavioral sciences may be expected. But even then I should expect a large interplay between managerial economics for reasons which extend beyond the current conjuncture of events and activities. First, I expect that new insights and new guides to research for understanding the behavior of economic variables will emanate from this quarter.[10] Second, I expect that the present emphasis on methodological research will continue and that this will result in new tools and approaches which will prove useful in other parts of economic analysis and teaching.[11]

I shall shortly return to these topics, but let me now refer to a recent report of an operations research symposium held in the Soviet Union which will, inter alia, help to supply perspective on the extent of the current ferment of activity as well as its potential effects on economic research. This symposium, which was held last spring, was attended by some of the most distinguished Soviet scientists and administrators and reached a positive conclusion on the value of continuing research and applications at all levels of their socialist planning: plant, industry and economy-wide.[12] Here, of course, the orientation towards a managerial formulation of economic problems and research is quite natural so that the surprise, if any, is rather that the interest in these developments should occur somewhat later in the USSR and somewhat earlier in the U.S. On the other hand, as we shall later indicate, there is reason to believe that many aspects of a free enterprise economy are also susceptible to interesting and effective treatment when restatements of traditional problems are made with an eye towards a more operational approach.

III. Some Predecessor Materials

When such activities as operations research and management science are joined to managerial economics, then the current state of the latter

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[9] See, e.g., J. G. March and H. A. Simon, *Organizations* (John Wiley and Sons, 1958).

[10] A suggestive discussion of the likely nature of these kinds of developments and their relations to economics may be found in H. A. Simon, "Theories of Decision-Making in Economics and Behavioral Science," *A.E.R.*, June, 1959. See also, H. A. Simon and A. Newell, "Heuristic Problem Solving: The Next Advance in Operations Research," *Operations Research*, Jan.-Feb., 1958.

[11] *Vide*, e.g., the recently published "Simulation: A Symposium," with articles by G. H. Orcutt, M. Shubik, G. P. Clarkson and H. A. Simon, in *A.E.R.*, Dec., 1960.

[12] The reference is to a report by V. Dadayan and U. Chernyak, "Mathematical Methods in Economics," *Ekonomicheski Nauki* ("Economic Science"), No. 3, 1960, which will shortly appear as a translation, with commentary, by P. Kircher and G. Ginsburgs in *Management Science*.

may be characterized as follows: (1) a vast and growing literature which is rooted in activities that cut across many of the traditional fields of science and management; (2) a tendency to blur the distinctions between these fields, as well as a tendency to blur the distinctions that are sometimes drawn between pure and applied research; and (3) a current preoccupation with methodology which is oriented towards operationalism and which is judged by its originators from the standpoint of its scientific value as well as its value for solving particular management problems.

I now designate the following three books for a brief examination: Marshall, *Industry and Trade;*[13] Grant, *Engineering Economy;*[14] and Dean, *Managerial Economics.*[15] (These selections are, of course, arbitrary. They are not intended to portray a lineal development from the past to the present but, rather, to help us highlight and understand some of the current developments in managerial economics and other analytically oriented approaches to management—or "management-able"—problems.)

Marshall's book[16] contains many shrewd observations on the managerial practices in the England of his time. The book was, in fact, preceded by an extensive survey which Marshall had conducted to acquaint himself with management practices in various British industrial firms. As set forth in the preface to Marshall's book, however, the main objective was to collect material that might have a substantive bearing on economics and little was done by Marshall either to disturb the "facts" of management practice as he observed them or to use the problems of practicing management as a source for new research material in either economic theory or methodology.[17]

Grant's book was, of course, conceived at a later date in an entirely different spirit and genre. It is of interest here, however, for the free use it makes of received economic knowledge—e.g., capital and interest theory—which is adapted to the ends of improving managerial and engineering practice. In contrast to Marshall's book, there appears to be no great concern here with improving the substance or the methods of economics, itself, viewed as a scientific discipline.[18]

[13] A. Marshall, *Industry and Trade* (London: Macmillan and Co., 1932).
[14] E. L. Grant, *Principles of Engineering Economy* (3ᵈ ed., Ronald Press Co., 1950).
[15] Cf., *supra*, footnote 1.
[16] K. Boulding prefers to regard the mathematician-economist, A. Cournot, as a more logical predecessor than Marshall. See his "The Present Position of the Theory of the Firm" in K. Boulding and W. Spivey, eds., *Linear Programming and the Theory of the Firm* (Macmillan Co., 1960).
[17] In this respect Marshall's book provides an interesting contrast with the still earlier survey results that are incorporated in C. Babbage, *On the Economy of Machinery and Manufactures* (London: Chas. Knight, 1832). Marshall's position, it should also be noted, is less extreme than the one stated by A. C. Pigou in his famous debate with Clapham about the "empty economic boxes." See T. C. Koopmans, *Three Essays on the State of Economic Science* (McGraw-Hill Book Co.), p. 185.
[18] These remarks should probably be qualified by reference to other works of Grant such as E. L. Grant and P. T. Norton, *Depreciation* (Ronald Press Co., 1949).

Another aspect that is pertinent to the current state of managerial economics can now be brought out by turning, once more, to Dean's book. The aspect referred to is Dean's constant awareness and perceptive use of his management studies as a possible source of new problem material for research in economics. This involves something more, of course, than merely noting novel problems (as they happen to be uncovered) or drawing attention to problems which many managements happen to regard as urgent or perplexing. It requires at least the following two additional features, which Dean supplies:[19] (1) An awareness of the state of economics which extends to an assessment of how it might be enriched by the problems under view as well as an informed judgment as to whether this state is such as to render any promise for developments that might prove useful in management. (2) It also requires a willingness (and ability) to effect restatements of the raw management problems so that they can be rendered in a form suitable for study by professional economists.

IV. *Some Carry Forward in Current Research*

The three kinds of activities that these summaries of the preceding books have helped us to uncover may be labeled (1) accumulation of factual information on the state of management practice, (2) use or adaptation of economic concepts (and methods) for solving management problems, and (3) location and specification of new problem materials for research. All three of these continue as part of the current state of managerial economics, although they are sometimes difficult to identify. One reason for this difficulty of identification is the rather heavy methodological-operational emphasis which, as already observed, is apparent in much of the current work. Another reason is that traditional boundaries between, say, economics and other disciplines tend to be blurred or extended in the way the research is conducted and the way that the material is developed and reported.

A case in point is A. Stedry's recently published study of research in budgetary and accounting (standard setting) practice.[20] Stedry's report is interesting, not only because it illustrates what has just been said, but also because it provides some indication of new directions of development. Even a casual reading will show that this study draws heavily from a wide variety of disciplines and methodology: economics, psychology, statistics, mathematics, etc. It also contains an extended critical discussion of the management and accounting literature dealing with budgetary (and standard setting) practice. From each of these

[19] See, for instance, Dean's discussion of the dynamics of new product pricing as given in *op. cit.*, pp. 419 ff.
[20] A. Stedry, *Budget Control and Cost Behavior* (Prentice-Hall, 1960).

backgrounds—science and management—Stedry is able to detect and develop a concept of "motivational costs" and hence a new series of problems and possible research avenues which were apparently not previously recognized (or at least not recognized in fully adequate detail) for their research potential in either the scientific or the management literature. To be sure, Stedry's work is in the area of intrafirm analysis. But it is interesting to note that he is aware of and also attempts to set forth some of the implications of his "motivational costs" for their more general bearing on price-cost mechanisms viewed as an information theoretic device for accomplishing the general ends of resource allocation in an individualistic, or decentralized, economy.

V. Some Current Developments and Their Frontiers

Mathematics. It is time now to attempt a more helpful characterization of some of these methodological developments and also to delineate some of the frontiers associated with current work. Many of these methodological developments are either mathematical in character or else bear a heavy mathematical flavor. To assist in providing some degree of discrimination, at least by emphasis, in these developments, we now borrow from Warren Weaver,[21] and distinguish three classifications according to whether the associated mathematics provides an advantage in dealing with problems where the following features are prominent: subtlety, complexity, and disorder. Then, with certain important exceptions,[22] one might observe that the greatest amount of recent progress appears to be in the second and third areas; e.g., algebra and probability as distinct from the first analysis. This has almost certainly been due, to some extent, to the preceding course of scientific development and, concomitantly, to recent important breakthroughs[23] which have made it possible to apply many of the received constructs and have thereby also stimulated further theoretical developments. The reverse process is also in evidence, and a particularly striking form of this is represented by developments which are motivated initially by a concern with managerial economics problems and depend vitally on a pre-existing set of operational mechanisms for their significance.

R. Gomory's solution of the so-called "Diophantine" linear program-

[21] These characterizations are drawn, with considerable liberties taken, from G. L. Thompson, "Computers and the Undergraduate Mathematical Training of Engineers," *Conference on Electrical Engineering Education* (Syracuse Univ., 1960). See also A. Charnes and W. W. Cooper, "Management Models and Linear Programming," *Management Science,* Oct., 1957.

[22] E.g., dynamic programming, as in R. Bellman, *Dynamic Programming* (Princeton Univ. Press, 1957).

[23] G. B. Dantzig's simplex method, as published in T. C. Koopmans, ed., *Activity Analysis of Production and Allocation* (John Wiley and Sons, 1951), is a good example.

ming problem is a recent case in point.[24] Here a particular methodological development has opened avenues for further managerial applications. It has also opened avenues for new mathematics and, more to the point here, it has helped to sharpen general issues in areas, such as welfare economics, which are central to general economics.[25]

It remains to be seen whether entirely new domains of economics may be opened from this, or other work in managerial economics, which will be discussed by others on this panel. The main points to be observed here are the following. First, some of these developments have already had a bearing on areas in economics which are sometimes regarded as being remote from managerial economics. Second, these developments may stem from the methodology research as well as the substantive work in managerial economics. Third, managerial economics has provided us with a convenient bridge for an exchange of progress and ideas with scientists in other disciplines and this, too, may prove to be an important frontier which managerial economics has opened. Finally, we should note that a greater call may yet be made on the mathematics of subtlety as the more obviously exploitable areas of managerial applications become exhausted, from a research standpoint, and new scientific knowledge makes it possible to turn to other kinds of management problems. (The control problems of management represent this kind of possibility.)

Other Developments. The literature of operations research and management science is replete with references to the electronic computer and its uses in simulation, gaming, and like devices that are now beginning to be adopted for use in economic analysis. It is not necessary now to go into these matters in detail since that has already been done by others.[26] I should merely like to say that the electronic computer has provided us with a powerful new tool, many of whose uses remain to be uncovered and explored. One way of relating this topic to the preceding discussion is to observe that the methodologies supplied from mathematics, statistics, etc., has enabled scientists to move into new problem areas where the psychological needs (e.g., of human understanding or aesthetics) which are impounded in the so-called fundamental "principle of parsimony" would otherwise have made the effort a forbidding one. The computer offers certain new possibilities of a like kind. First, observe that a principle of parsimony for a machine is not likely to be on all fours with a principle of parsimony for humans. Second, observe that a division of labor is also possible. In particular,

[24] R. Gomory, "An Algorithm for Integer Solutions to Linear Programs," Princeton-IBM Mathematics Research Project, Technical Report No. 1, Nov., 1958.
[25] See R. Gomory and W. J. Baumol, "Integer Programming and Pricing," *Econometrica*, July, 1960.
[26] See, e.g., the references cited in footnote 11, *supra*.

the more complicated and tedious parts of the given research tasks that are associated with some of these previously unexplored problems can be allocated to the machine; in principle at least, it should be possible to do this in a way that allows the human to retain only those parts of the research task that are suited to his talents and limitations. Moreover, this extends to the problem as well as its solution as when, for example, its ill-structured parts are allocated to the machine and the well-structured parts (as they become available) are submitted to further analysis by the human mind.[27] There is, of course, no reason why this cannot be done in a way that allows for co-operative play between the computer and all of the traditional aids that can be supplied via mathematics, substantive theory, etc., so that, from this standpoint, the computer may be viewed as allowing us to give these approaches a wider and freer play over a broader range of topics.

These kinds of developments should also make it possible to draw upon a wider variety of subject matter (and methodological) disciplines. This can be illustrated, for example, by reference to the "behavioral theory of the firm" research that is being undertaken by R. M. Cyert, J. G. March, and their associates. This work has been predicated on a behavioral science-organization theory approach whose direction is determined by reference to key economic variables in an effort to analyze such things as price, production, plant expansion, and marketing decisions at the level of the individual firm. Electronic computer simulations have here been joined with economic and general social science theorizing as well as a variety of tools drawn from each.

In keeping with our preceding discussion, we may also note the new kinds of problems and contacts with other disciplines and points of view that appear now to be emerging from this "behavioral theory of the firm" work. For example, one of the questions which this research has been led to pose may be phrased as follows: Given certain observed (or observable) data of, say, a price-production variety, what kinds of organization arrangements could have produced the indicated behavior? Conversely, given certain classes of organization structures, what are the kinds of behavior patterns (hence observations) that can be predicted? This kind of problem is clearly related to the standard one in biology—e.g., in neurophysiology—of inferring structure from function and vice versa.[28]

Alternate Approaches to Traditional Problems. Evidently this kind of work is opening frontiers and helping to establish contacts which appear to be promising in a variety of ways. But our exploration would

[27] See footnote 8.

[28] See especially their recent work on organization bias as reported in R. M. Cyert, J. G. March and W. H. Starbuck, "Two Experiments on Bias and Conflict in Organizational Estimation," *Management Science*, Jan., 1961.

not be complete if we failed to observe that some of the recent work on already known problems in economics has also been influenced by these developments. Consider, for instance, some of the recent work by H. Theil,[29] C. C. Holt,[30] and others.[31] These represent "policy studies" (in the high traditions of economic analysis) which deal with problems such as economic stability, growth, etc. But one cannot fail to note a difference in the approach that is utilized in this more recent work. It is much more operational in its orientation. The deep and subtle aspects of "understanding" that are dealt with in, say, traditional welfare economics are skirted in favor of criterion functions—e.g., quadratics— which have certain broad and appealing properties: they are simple and readily understood; they provide access to a few critical and easily managed variables that are available (or can be made available) to the policy-makers of an economy; and they produce desirable kinds of economic behavior over certain ranges of activity and policy where agreement can be readily secured from persons with varying points of view and (somewhat fuzzy) value positions. There are, of course, other properties which are desirable (e.g., properties of certainty equivalence) in these functions and here rather deep research may be necessary. But the point to be made here is rather the differences that are apparent in this approach—e.g., its operational flavor—as distinguished from the approaches which have distinguished much of the preceding research on analogous problems. It will perhaps help to bring the point somewhat more to the fore if I note the tendency in this more recent work to avoid refined distinctions between different kinds of cycles, seasonals, random shocks, etc. The tendency is rather to lump all relevant sources of instability together and to judge the model and its associated criterion function in terms of its over-all behavior.

It is true, of course, that even these ideas are not entirely new. Some years ago, for example, O. Lange constructed a rather elaborate model which rested mainly on the then generally available tools supplied from the field of differential equations. Hopefully, this model could be used by legislators, say, to examine alternate policies and arrive at rational judgments in terms of their own value systems. The model was a cumbersome one, at best, and so far as I know it was not even tested by reference to the numerous ranges of empirical phenomena that would need to be considered. This does not mean, of course, that Dr. Lange was not mindful of these problems. Rather the lack of suitable tools (e.g.,

[29] H. Theil, *Economic Forecasts and Policy* (Amsterdam: North Holland Pub. Co., 1958).
[30] C. C. Holt, "Linear Decision Rules for Economic Stabilization and Growth," Carnegie Inst. of Tech. Res. Report, 1960.
[31] See, e.g., A. W. Phillips, "Stabilization in a Closed Economy," *Econ. J.*, June, 1954. See also A. Tustin, *The Mechanism of Economic Systems* (London: Heinemann, Ltd., 1953) for material in a parallel vein.

as exemplified by electronic computers) made the prospect of such testing rather uninviting without rather firm guarantees of adequate resources. The latter was not likely to be forthcoming, however, unless the requisite policy organs were persuaded that the results would be useful to them. On the other hand, such persuasion would be difficult to produce from the evidence supplied only by a few qualitative analyses of a rather abstract model.

It does sometimes happen, however, that an idea once deemed unworkable can, in fact, have its character changed in the contexts of new methodologies and developing experiences. The world, too, can change under this kind of impact. For instance, it is no longer an occasion for surprise when problems of high-level policy in government and business are now submitted to analytically oriented scientific research approaches. This, too, is a frontier in managerial economics. It appears to be an expanding one and it is, perhaps, the biggest frontier of all.

WHAT CAN ECONOMIC THEORY CONTRIBUTE TO MANAGERIAL ECONOMICS?

By WILLIAM J. BAUMOL
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What to me is one of the most significant aspects of economic theory for management science was brought out very clearly in a talk I had some time ago with a biologist friend of mine. This biologist is an eminent authority on clock mechanisms in animals. There is a remarkable and well-known periodicity in the behavior of a large variety of animal species—in fact, probably among all of them. To illustrate the point, the emergence of adult fruit flies from their pupae usually occurs shortly after dawn. Even if the flies are placed in a darkened room whose temperature, humidity, and other evidences of passage of time are carefully controlled, they will continue to emerge from the pupae at just about the same time day after day. However, if after being kept under these controlled laboratory conditions they are suddenly shown some light in order to produce the effect of a false dawn, there is a permanent shift of phase and, after some transient behavior, they will change the time at which they emerge from the pupae to that corresponding to the dawn which they were last shown. This suggests that there is a very definite way in which these animals can tell the time; that is to say, in which they can recognize when twenty-four hours are over, even though there is nothing conscious about it.

Of course a clock mechanism suggests periodicity, and periodicity, to any good cycle theorist, suggests difference or differential equations. And in fact, after this biologist had been working on the subject for some time, he became aware of this possibility and set out to find a mathematician who could help him to determine an appropriate equation. This was done, a relationship was fitted by statistical methods, and it turned out that it was appropriate to use a nonlinear differential equation. It was found that one such equation could fit a great variety of the data which this man had available. Not only could it do that, but with the aid of the equation he was able to make a number of interesting predictions which were subsequently very closely confirmed by data which he was able to collect.

Here is where we come to the point of the story—the contrast between the situation of the biologist and that of the economic theorist—for the biologist who had obtained a very nice relationship on the basis of empirical data was totally unable to give any sort of analytical ex-

planation of what he had. He had absolutely no model on which he could base a derivation of his mathematical relationship. We may, perhaps, generalize by remarking that biologists, with some notable exceptions, have data without models, whereas we in economic theory have models which usually are created without data. And in this way we have summarized one of the economic theorist's greatest weaknesses and one of his greatest strengths.

I would now like to emphasize the latter, the more pleasant, side: the fact that the economist is an expert model builder. Indeed, there are very few disciplines which produce model builders with such practice and such skill. This, I think, is one of the most important things which the economic theorist can contribute to the work of management science. In management science it is important—in fact, absolutely essential—to be able to recognize the structure of a managerial problem. In order to be able to analyze it at all and to be able to do so systematically, it is necessary to do several things: first of all, to undertake a judicious simplification—an elimination of minor details which are peripheral to the problem and which, if included in the model, would prevent any successful and systematic analysis. Second, it is important to capture in a formal statement the essence of all the interrelationships which characterize the situation, because it is only after stating these interrelationships so explicitly that we can hope to use the powerful techniques of rigorous analysis in the investigation of a managerial problem. It is the model which incorporates both these features; it is the central focus of the entire analysis which must capture the essence of the situation which is being investigated.

Thus, in any of the complex situations which are encountered in the systematic analysis of management problems, model building is a critical part of the investigation. Problems as diverse as the optimal size and composition of a department store product line or the location of a company's warehouses have one thing in common: their complexity—which arises to a large extent out of the network of interrelationships among their elements. An increase in the number of items carried in a store reduces the capacity for carrying stocks of other items: on the one hand it makes it more likely that the customer will find what she wants when she enters the store; on the other she may find more often that although the store usually carries what she desires, it happens to be out of it temporarily. The length of time a customer must search for an item is affected by a change in product line; the likelihood of "impulse" purchases is also affected, etc. The drawing together of such a diversity of strands is the major function of the model, without which most of our tools will not function. Moreover, in my experience it is not atypical that nearly half of the time spent in the investigation of

such a problem is devoted to model building—to capturing the essence of the situation in a set of explicit relationships. For there are no cut and dried rules in model building. It is essentially a matter of discovery, involving all of the intangibles of discovery—hunch, insight, and intuition, and no holds are barred. Only after the model has been built can the problem sometimes be reduced to a routine by use of standard rules of calculation.

To my knowledge there are few classroom courses in this critical skill of model building, and, because it has no rules, it cannot be taught like trigonometry or chemistry. But apparently it can be learned by experience. And, as I have said, the economic theorist has had a great deal of experience in the construction and use of such models. When he employs some differential equations, you can almost be certain that he has derived them from a model which he built, not, like the biologist, from some data which he has collected.

This, then, is one of the major contributions which the economic theorist can, in my opinion, make to managerial analysis. It is, however, a skill and a predisposition that he brings with him, not a series of specific results.

This takes me to the second major point that I wish to make: the other way in which I think economic theory can be helpful to management science. I believe the most important thing a managerial economics student can get out of a course in economic analysis is not a series of theorems but rather a set of analytical methods. And for that reason I think it is far more important for him to learn the basis of these theorems, their assumption and their methods of derivation, than to end up with a group of conclusions. I can say quite categorically that I have never encountered a business problem in which my investigation was helped by any specific economic theorem, nor, may I add, have I ever met a practical problem in which I failed to be helped by the method of reasoning involved in the derivation of some economic theorem.

One of the major reasons that the propositions of economic theory are not directly applicable to management problems is that the theory does not deal with the major concerns of the businessman. Product line, advertising, budgeting, sales force allocation, inventory levels, new product introduction are all relative strangers to the idealized firm of value theory whose major concern is price-output policy. Certainly there is little in the theoretical literature which refers directly to the warehouse location or the department store product line problems which were mentioned previously.

Even where more familiar theoretical matters, such as pricing problems, arise in practice, the results of the theory provide only limited

help. This is because theorems in economic analysis deal with rather general abstract entities, with firms which have the peculiar and most interesting characteristics of actual companies eliminated from them in order to enable the analyst to draw conclusions which apply to the entire economy and not just to one or several particular firms. As a result, when attempting to apply these theorems, one finds that they have abstracted some of the features it is most essential to retain in order to analyze the specific situation with which one is faced in the market. The theory offers us fairly general admonitions, like the one which tells us that marginal cost must equal marginal revenue if we are to maximize profits—surely a statement which is not very much of a guide in application. I repeat that in my applied work I have never found any occasion to use either this theorem or any other such specific proposition of economic analysis.

But I have often found it absolutely essential to use the techniques of marginal analysis as it occurs in the theory of the firm, the theory of production, and in welfare economics. Several times I have even found it helpful to use the techniques and derivations of some of the elasticity theorems. This last illustration perhaps merits a little expansion. It may appear extraordinary that the elasticity theorems were of any use in application at all for they would seem to provide the ultimate illustration of tools whose use requires the availability of extensive data. However, the point I am making is that it was not the theorems but the methods of analysis and derivation which were employed. For example, an analogue of the elementary proposition that unit elasticity is the borderline between increasing and decreasing total revenue in response to a decrease in price can be applied in other situations. In fact, it is precisely because of the lack of data that it often becomes necessary to decide just where such break-even points occur, and in a number of cases I have found that the ability to prove that this critical point is sufficiently beyond what may reasonably be expected is an adequate substitute for the availability of data. Thus knowledge of the method of derivation of the theorems—and, indeed, of the spirit of the theorems themselves—often enables one to do things without data which otherwise would be pretty much out of the question.

But this is not the major point. If it is true—and it certainly has been true in my experience—that every firm and every managerial situation requires a model which is more or less unique, none of the standard theorems is going to fit in with it. It will be necessary, in effect, to derive special theorems which enable one to deal with that specific situation. Here one is helped, then, not by the generalized propositions which have been developed by the theorist, but by the

methods which have enabled him to achieve his results which show us how analogous conclusions or analogous analyses can be conducted for the problems at hand. It is for this reason that I make my plea about the teaching of economics and economic theory to the managerial economist. This plea is not only that economic theory should be taught to the business student but that it should be presented to him pretty much as it is taught to the liberal arts student, with the emphasis not on a series of canned conclusions but on the methods of investigation on the derivations behind the results—on the analytic tools and methods.

There is a third way in which economic theory can help in managerial analysis—and, perhaps strangely, here the more elementary concepts of economics are primarily involved or, rather, concepts which though relatively sophisticated are used in a very elementary way. These elementary concepts can imbue the economist with habits of thought which enable him to avoid some significant pitfalls. For example, consider the case of external economies and diseconomies. How much can familiarity with this concept tell us about the dangers involved in directing one branch of an enterprise to maximize its profits in disregard of the effects of its actions on other parts of the firm! Similarly, we economists are made very sensitive by marginal analysis to the perils of resource allocation by average cost and profit—resource allocation rules of thumb which are so frequently encountered in business practice. Such bits of reasoning once led one of my colleagues, who was reviewing some of the cases cited in the literature of managerial analysis, to remark that he was amazed at how often this reading had forced him to recall his sophomore economics!

To summarize, then, I have suggested very little by way of concrete contribution from economic theory to managerial economics. With some exceptions, I have not said that this particular result or that particular body of discussion is essential or even particularly helpful for the managerial economist. I have been able to offer no illustrations of managerial problems in which I was able to use very specific pieces of the body of economic analysis. But this is right in line with the very nature of my major point: the assertion that a managerial economist can become a far more helpful member of a management group by virtue of his studies of economic analysis, primarily because there he learns to become an effective model builder and because there he acquires a very rich body of tools and techniques which can help him to deal with the problems of the firm in a far more rigorous, a far more probing, and a far deeper manner.

WHAT CAN MANAGERIAL ECONOMICS CONTRIBUTE TO ECONOMIC THEORY?

By Charles J. Hitch *and* Roland N. McKean
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As background for this discussion, let us review the customary distinction between economic problems, on the one hand, and management problems, on the other. The distinction is really between problems of choice at different levels. Traditionally, economic problems have related to the allocation of resources among broad uses, taking efficiency within firms for granted, while managerial problems have pertained to resource use within individual firms (or other organizations).

The line of demarcation is a blurred one, of course. For one thing, it is hard to define a firm precisely in view of the diversity of organizational structures, and for another, the whole subject of inputs, including behavioral inputs, is a problem that is necessarily of concern to economists. If one learns more about the behavior of individuals or firms, it may affect hypotheses about resource allocation among broad uses. Certain branches of physics have a somewhat analogous relationship. Theories about the behavior of large masses and the spectra emanating from the stars influenced our understanding of the atom; and there was in turn a feedback from the study of the atom upon our understanding of the orbits of and emanations from various celestial bodies.

Thus the dividing line between these two subjects is not clear cut. Both deal with problems of choosing among alternative ways of using resources—above the level of the firm in one case and within the firm in the other. Both involve calculations for economizing. Let us turn now to a more specific aspect of their relationship: the potential feedbacks from management economics to economic theory.[1]

I. *Our Business Behavioral Inputs*

One part of economic theory to which managerial economics may contribute comprises the inputs concerning business behavior. In much of our theorizing we have fallen into the lazy habit of making convenient but naïve assumptions about individual, business, and government behavior. There are at least two good reasons for re-examining these behavioral assumptions.

One reason is that examining assumptions underlying theories may

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[1] Professor H. A. Simon also has addressed himself to this question, as well as to the possibility of other impacts on economic theory, in his article, "Theories of Decision-Making in Economics," *A.E.R.*, June, 1959, pp. 253-83.

lead us to new and promising hypotheses to be tested. One does not put a monkey in front of a typewriter, have him grind out hypotheses at random, and then proceed to test them. As Bob Newhart has reminded us, this procedure, with good luck, results in propositions like: "To be or not to be; that is the gesinderplatz." In devising theories worth checking, one looks at the real world and asks, "What hypotheses are plausible? What abstractions will still fit reality well enough to yield a useful theory?" Thus in screening hypotheses initially, we do appraise them in terms of the realism of their assumptions.

Another reason for re-examining behavioral assumptions is that we continue to appraise theories, long after any initial screening, partly by the reality of their assumptions. Some have argued that the test of a theory is its ability to make useful predictions, not the realism of its assumptions. This point appears to be valid for theories yielding predictions that can be checked, such as econometric models of cyclical fluctuations. Unfortunately, some important economic models yield insights on the basis of which we predict outcomes, but outcomes not subject to adequate empirical check. An example is the theory that a private enterprise economy tends (or does not tend) toward Paretian optimality. How does one test such an hypothesis? By seeing if subsidiary implications are consistent with the facts? But that is analogous to testing for the accuracy of assumptions! In the end we are forced to appraise some theories according to the accuracy of side implications and of assumptions.

Let us examine the business behavioral inputs to economic theory, then, and see how managerial economics may affect those inputs. The usual assumption about business behavior has been that firms seek to maximize profits. This may be all right for some purposes. It seems to be valid in some rather unrealistic circumstances; namely: if there is no risk or uncertainty (including uncertainty about distant timestreams of costs and receipts); if all production is carried on by purely competitive firms; or if the management of each firm, however sheltered from competition, is interested solely in profits. If there is vigorous competition and certainty, it would appear that the forces of natural selection insure that surviving firms behave as if they are trying to maximize profits.[2] At best, however, one must be cautious in generalizing about natural selection. Where there is a diversity of organizational forms, it is not really clear what types of behavior can survive.[3]

In any event, the main conditions for natural selection of profit

[2] Armen A. Alchian, "Uncertainty, Evolution, and Economic Theory," *J.P.E.*, June, 1950, pp. 211-21.
[3] Sidney G. Winter, Jr., "Economic Natural Selection and the Theory of the Firm," a paper presented at the meetings of the Econometric Soc., Dec., 1960.

maximizing firms do not prevail. Pure competition does not exist in much of the economy. There is production by government and production contracted for by government, often by means of some form of cost-plus contract. Many activities are carried on by nonprofit corporations or other public authorities. Public utilities and a few other industries are deliberately sheltered from competition. Moreover, in the real world there is dynamic change and disequilibrium, not equilibrium. Thus there is a great deal of leeway for the management of various organizations to seek goals other than profit maximization.

Do they have other goals? There is ample evidence that they do—that firms are concerned about shares of the market, about "satisficing" rather than maximizing. Especially when management is separated from ownership, a multiplicity of goals develop.

Much more important, though, management could hardly pursue profit maximization, even if it wanted to do so. For maximum profits is not even a meaningful concept in a realistic environment. We are so bemused by static theory that we keep forgetting how ambiguous this concept is in a world of change and uncertainty. To take the most obvious ambiguity, consider uncertainty about the final outcomes of alternative courses of action. If alternative A could yield anything from a million-dollar profit to a half-million-dollar loss, while alternative B would almost certainly produce a $250,000 profit, what is the profit maximizing course of action? Equally obvious is the ambiguity caused by elements of gaming. If the best policy for one firm depends on how other firms react, what course of action constitutes profit maximization? Still more pervasive are the effects of the uncertainties that harass decision-makers at all levels of the firm and at every step of the way—uncertainties about everything from the impacts of an advertising campaign to the effectiveness of a new sweeping compound. There is an infinity of alternatives to be considered, and even the costs of acquiring information about them are uncertain.

What do firms do in the face of these unknowns? How do they behave? Clearly they suboptimize—breaking out many decisions to be made separately from others, delegating decisions, adopting crude rules of thumb, sometimes taking blind stabs. Furthermore, it is doubtful that the surviving firms can appropriately be described as profit maximizers. For some of the effects of such an environment on the survival of firms, see Sidney Winter's paper, "Economic Natural Selection and the Theory of the Firm."[3]

In this situation, how does managerial economics contribute or show promise of contributing to economic theory? One way is simply by emphasizing the extent to which our behavioral inputs are inadequate

—the extent to which firms pursue goals other than profits and have to cope with uncertainties.[4] The mere fact that firms have been making use of operations research and management economics suggests how elusive the path to profits is. Either those firms were operating inefficiently in the past or they are operating inefficiently when they purchase operations research.

More significantly, many management analyses have apparently paid off. Certain petroleum companies pay regularly for linear programming solutions to problems of blending and production scheduling. The blending of animal feeds is another well-known application. One manufacturer of electrical equipment markets a line-balance computer to allocate power output among plants. Gas and milk companies apparently believe that more sophisticated solutions to their "traveling servicemen" problems will increase profits. Managerial economics has contributed to the more efficient use of drag lines in strip-mining, to more economical beneficiation of ores, and to more efficient mining operations of other sorts. Simulation of plants such as metal-working shops is apparently paying its way in connection with shop modifications.[5]

Thus such studies have been underlining the facts that businesses have criterion problems, that they often use rough rules of thumb, that they must cope with the lack of information and the presence of vast uncertainties—that, in short, our business behavioral inputs have been oversimplified. Such studies emphasize that the assumption of profit maximizing behavior is at most a first approximation and that we should not be content to stop with first approximations. They emphasize that we should explore the formulation of a more general and more complex theory of the firm. As one step, we might postulate utility maximizing business units, as Alchian and Kessel have proposed.[6] In just the right circumstances, these units necessarily become profit maximizing (or loss minimizing) organizations. (In fact, Alchian and Kessel suggest that one might test for monopolistic power by looking for the thickest carpets, the highest percentage of beautiful secretaries, and other evidence of nonprofit maximizing behavior.)

To subsequent steps—the development of improved behavioral inputs wherever utility maximizing firms are not necessarily profit maxi-

[4] Charles Hitch, "Uncertainties in Operations Research," *Operations Research*, July-Aug., 1960, pp. 437-45.

[5] For some of the applications, see D. G. Malcolm, "Bibliography on the Use of Simulation in Management Analysis," *Operations Research*, Mar.-Apr., 1960, pp. 169-77. For more examples of other types of operations analysis, see the references at the end of Robert Dorfman's article, "Operations Research," *A.E.R.*, Sept., 1960, pp. 575-623.

[6] A. A. Alchian and R. A. Kessel, "Competition, Monopoly, and the Pursuit of Money," to appear in the forthcoming volume of papers presented at the Universities-NBER Conference on Labor Economics in Apr., 1960.

mizers—managerial economics may contribute in a more positive
fashion. By forcing us to observe business practices more closely, it
may show us more about how firms really operate. And better be-
havioral inputs may in turn yield better theories; for example, hy-
potheses that explain more satisfactorily how prices move in recessions
such as those of the fifties.

Studies of business and government operations, often reported on in
publications like *Management Science* and *Operations Research*, are
beginning to hold out the promise of such improved behavioral inputs,
sometimes explicitly but more often implicitly. For examples, see the ref-
erences in H. A. Simon's article in the *American Economic Review* last
year and the papers from the 1955 Social Science Research Council
Conference held at the Carnegie Institute of Technology.[7] Analyses of
the behavior of oligopolies may make game theory and bargaining
theory in economics more useful than they have been to date. Studies
of information gathering and data processing may shed light on murky
aspects of large firms' decision-making processes. At a more conven-
tional level, management research is increasing our knowledge of cost
and production functions. Research on firms' inventory policies, equip-
ment replacement, and sequential decision making may produce sig-
nificant impacts on dynamic theories, such as models of inventory
cycles and inflationary processes. Analyses of advertising programs and
research and development policies may influence hypotheses about
changes in the production function. Studies of firms' decision processes
and research and development strategies may modify theories about
business behavior in the face of uncertainty. Interest in management
science and familiarity with concrete situations may shift the emphasis
in welfare economics from defining the conditions for optimality to
searching for "improvements"; that is, for courses of action that are
better than other specified policies even though it cannot be proved
that they lead toward an over-all *optimum optimorum*.

A different sort of study of "management" behavior that deserves
mention is Anthony Downs's inquiry into the economics of political
parties.[8] Political parties are nongovernment agencies that seek utility
but not profits, being similar in some ways to nonprofit corporations.
Downs's study helps show the two-way relationship between eco-
nomic theory and management economics: it indicates how eco-
nomics can contribute to the analysis of an organization's practices and
suggests how better understanding of those practices may contribute
to a theory of organizational behavior.

[7] Simon, *op. cit.;* and M. J. Bowman, ed., *Expectations, Uncertainty, and Business
Behavior* (SSRC, 1958).
[8] Anthony Downs, *An Economic Theory of Democracy* (Harper and Bros., 1957).

All of these potential contributions would come about through the provision of more appropriate behavioral inputs. In some instances, managerial economics may bring firms' behavior more nearly into line with present inputs. In others, it may reveal more clearly the sectors of the economy in which we need better behavioral assumptions. And, finally, it may help us devise improved inputs regarding those sectors.

II. *Government Units' Behavior*

Another group of organizations that can be regarded as utility maximizers but not profit maximizers are government agencies—various units of government at federal, state, and local levels. Whether the study of their behavior is economics or managerial economics is not at all clear. Analyses of specific governmental operations or problems of choice are extremely similar to analyses of business operations and management problems. If we can reach generalizations about governmental behavior, however, they would constitute an economic theory of government expenditure.

Government is now a significant sector of the economy. We live in a mixed economy, and economics should be concerned with the performance of mixed economies and with ways to improve their performance. As noted earlier, we know far too little regarding the behavior of business firms, and we would be still more at sea if it were not for our partial understanding of "natural selection." When we turn to government, we do not have even that aid, because we understand even less about the process of natural selection in government. Perhaps the time has come to develop a theory of government behavior to supplement the theory of the firm. The research of several economists—among them Ronald Coase, James Buchanan, and C. E. Lindblom—bears on the development of such a theory.[6] Because of the importance of the subject, a great deal more work on it is warranted.

The economics of the governmental unit could be important in connection with several types of policy decisions. First, it would be pertinent to deciding whether an activity should be conducted by private firms or public agencies. Existing theory tells us that numerous activities will be conducted inefficiently if left to private firms, because of, say, imperfect competition or external economies and diseconomies. There is often a tendency—perhaps because we have no economic theory of government behavior—to assign such activities, without much question, to a government agency. We apparently simply assume that a public agency automatically behaves in the public interest. As

[6] R. H. Coase, "The Federal Communications Commission," *J. of Law and Econ.*, Oct., 1959, pp. 1-40; J. M. Buchanan, "Politics, Policy, and the Pigovian Margins" (unpublished); C. E. Lindblom, *Bargaining: The Hidden Hand in the Government* (RAND Corp., RM-1434-RC, Santa Monica, Calif., 1955).

George Stigler has pointed out, this is like the decision of the emperor who was to judge the performance of two singers and awarded the prize to the second after hearing the first.

Next, the economics of governmental units might help show what methods, techniques, or "systems" should be used to carry out certain activities assigned to the public sphere. The analogy to management science is clear, but the choices, for example, in agricultural policy or among modes of transportation, have always been regarded as problems of economics.

Another type of policy decision on which a theory of government behavior would have a bearing is the choice of the institutional framework in which a governmental activity is conducted. Often the costs and rewards confronting an agency pull it irresistibly toward wrong decisions from the nation's standpoint. With a better understanding of organizational behavior, we might be able to devise institutional arrangements such that an agency's costs and rewards (including the costs of offending certain groups and the rewards from pleasing other bargainers) would more nearly coincide with the costs and rewards to the nation.

What does management economics have to do with developing an economic theory of government behavior? It may have a great deal to do with it. Management economics is concerned with the internal or management problems of governmental units as well as those of business firms, and studying management problems inside government is almost a *sine qua non* of developing an economic theory of government behavior. Operations analysis for a governmental unit reveals much more vividly than armchair speculation the criterion problems that beset government officials.

Analyses of managerial problems in defense planning, in resource development, and in local activities such as urban transportation systems or the operations of port authorities suggest how complex the decisions are and how little we know about government units' behavior. At the same time many of these analyses of governmental operations have pointed the way to greater efficiency. Eventually the cumulative effect of such studies may provide improved insights into governmental behavior and therefore improved behavioral inputs in economics.

III. *Managerial Economics and Normative Economics*

Finally, management economics should be able to contribute in another way toward the achievement of the aim of normative economics. Speaking somewhat loosely, we can say that the objective of normative economics is the maximization of the value of output in a

national (or regional or world) economy. As noted before, management research can contribute if it can increase the efficiency of business and governmental units. This contribution would take the form of improvements, not in economic theories, but rather in production functions. Here we refer to managerial economics that pays off, such as the recent research on a barge company's scheduling policies[10] or, for a governmental unit, the studies of traffic for the Port of New York Authority.[11] A type of analysis that is quite promising for both firms and government is Allais' application of statistical techniques to prospecting for minerals.[12]

To view successful managerial analysis as being itself a feedback to economics may conflict with traditional definitions of economics. Yet, as pointed out before, the dividing line between economic and management problems is a hazy one, and the concern of economists about productivity in the narrow sense goes a long way back. All means of increasing productivity are of interest in economics.

Does this line of argument "prove too much"? Does it expand the province of economics unreasonably so as to include, for instance, analysis of the consumer and improvement of his efficiency—say by helping him to be well informed and to choose "rationally"? Does it expand the domain of economics to include the analysis and promotion of technical progress—tasks normally left to psychologists, scientists, and engineers?

Perhaps the line of argument does lead in that direction, but it is hard to see much force in objections to this. To achieve the results that are really desired, we should draw the boundaries between disciplines more pragmatically, less arbitrarily. The revelant questions are: (1) What is important for increasing the value of output? (2) Can trained economists and their kit of tools be useful? (3) Can management economics be helpful? Whatever tools, disciplines, and activities can increase the value of output should be used. To us, results that expand national output seem like economics. But it does not really matter whether some result is called management science or a feedback to economics. What would matter would be the failure to use all of the tools or skills that can increase total output. Failure to use part of them because of arbitrary boundaries would be, to say the least, a sterile sort of traditionalism.

[10] George G. O'Brien and Roger R. Crane, "The Scheduling of a Barge Line," *Operations Research*, Sept.-Oct., 1959, pp. 561-70.

[11] Leslie C. Edie, "Review of Port of New York Authority Study," *Operations Research*, Mar.-Apr., 1960, pp. 263-77.

[12] M. Allais, "Method of Appraising Economic Prospects of Mining Exploration Over Large Territories: Algerian Sahara Case Study," *Management Science*, July, 1957, pp. 285-347.

DISCUSSION

JULIUS MARGOLIS: Sciences do not develop in a simple systematic fashion. At the peripheries of the academic disciplines there exists a ferment of inventors who in time will be proclaimed cranks, faddists, or innovators. Even when we feel confident that the inventors are making significant contributions it is always difficult to separate the froth from the solid potentials of an innovation. The endorsement of four such eminent economists as represented on our panel clearly attests that managerial economics has reached acceptability, but the persisting need of symposia to explain, from different perspectives, its substance and contribution indicates that the excitement has not subsided. The boundaries of the field have not been formed. Departments have not begun to hire managerial economists.

The field has so many diverse aspects that it is not strange that it has not become stable. To many it is no more than operations research and then vaguely defined as the application of the scientific temperament to industrial and business problems. To others it is the application of a specific set of models to these problems. The one thing common to the many practitioners as evidenced in the three papers is a commitment to the solution of problems of complex organizations. Though this is the present burden of managerial economics, it is probable that its impact will be greater both on the range of problems included in economic analysis and on the structure of theory.

Economics has been concerned with rather gross qualitative statements of tendencies. Though the arguments seem refined and rigorous, the analytical tools have not allowed the economist to specify with sufficient precision the predictions of his theory in the actual world or even in experiment. This freedom of the restraint of verification has permitted a flourishing of theory but it also meant an irresponsibility in its development.

Managerial economics with its young traditions of specifying the technology not in generalized production functions but in specific processes; of identifying and measuring the goals of an organization rather than assuming a simple profit maximizing goal; of considering the role of information and ignorance instead of certainty; of treating the organizational attributes of the firm as a constraint or as a variable instead of collapsing the firm into a single personality; of finding statistical constructs to enable measurement with analysis rather than remaining satisfied with unmeasured concepts; of welcoming mathematical and computer techniques to handle complex systems—all will help considerably in studies far beyond the operations of the firm.

Understressed by all of the papers is the contribution of managerial economics to positive analysis. The needs to develop the normative model to the point where it may be found acceptable by the client and prove itself useful to his operations, necessitates an understanding of behavior which is bound to influence heavily our known models. In the past, the major role played by business economics has been the analysis of the market for the firm's outputs

or inputs and the estimation of a few variables, especially costs. The business economist had little to offer to theory development, but the managerial economist has already made contributions to theory in areas as uncertainty, investments, finance, etc.

Another area understressed by the panel is the major role that the developments in managerial economics will have on econometrics. The sophisticated use of statistical concepts, mathematical models, and the availability of numbers has excited the interest of the econometricians to managerial economic problems. It is likely that the major impact on economic analysis will be through econometrics.

Experience with complex models may have a profound effect on the whole set of traditions within economics. The lessons learned in managerial economics will spill over into other sectors. Messrs. Hitch and McKean rightly stress as an area of influence the nonprivate sector of the economy, which has been declared off-limits because of its insulation from the market. Other areas of complex problems which will be benefited are those dealing with the organization of the metropolitan economy, transportation, and even the total economic organization. It would be presumptuous to attribute all of these possible developments to managerial economics, but the area has certainly contributed.

Professor Baumol is certainly correct in stressing the role of optimizing models in managerial economics, but it is not clear that a course in economic theory will serve the desired purpose. It is probable that modern statistics can be as efficient a vehicle for apprenticeship in optimizing models as is economic theory. Theory is still more interested in theorems of equilibrium positions than in the analysis of behavior. The concentration on theorems is being reduced, but currently training in theory has an inflexibility which is restraining the development of analytical models of behavior.

Economic theory in its current form will prove useful in an area neglected by our panel. The managerial economist probably will concentrate on the areas of the firm closest to the market. And it is in those areas of market adjustments where the special expertise provided by economic theory will have a major pay-off.

FRANCO MODIGLIANI: I find the task of commenting on the papers presented at this session a very difficult one. Methodological discussions are seldom exciting, and among such discussions, the subset labeled "What can Discipline X contribute to Discipline Y?" is generally the dullest. The authors of the papers seem themselves to have had a rather difficult time finding something worth while saying on this subject, although, in view of the handicap, I feel they have managed exceptionally well. But this only makes my task of discussant the more difficult, for there is little to disagree with in what they have said. However, by working very hard at it, I can manage to find a few points of disagreement, mostly with Messrs. Hitch and McKean. And in line with the character of the session, the disagreement is largely one of a methodological nature.

According to these authors, the relation between economic theory and

managerial economics is that "both deal with problems of choosing among alternative ways of using resources—above the level of the firm in one case and within the firm in the other." The definition of economic theory implied by this statement leaves out a broad area which has been, and still is, my focal point of interest; namely, positive economics, which is concerned not with the most efficient use of resources but rather with understanding and explaining how our economic system works. In contrast, managerial economics is to me essentially a normative science: its aim is to give advice to the management of an organization—mostly of business firms but not excluding others, public or private—on how to operate more effectively in pursuing the goals of the organization in question. These goals may or may not be reducible to the making of profits, but in any event, the problem with which the managerial economist deals can usually be reduced, broadly speaking, to that of getting the most for the least.

In spite of the difference in goals, I have referred to managerial economics as a science because it shares in common with positive economics and other positive sciences two major methodological features. The first is the reliance on "models," which must be simple in order to be manageable and which therefore selectively oversimplify the concrete situation though endeavoring to retain those few elements which to the scientist seem the essential ones; i.e., in his view make the model sufficiently "realistic." In positive science, the model is used to deduce verifiable implications or predictions and to state procedures for such verification. In the case of managerial economics, on the other hand, these models typically distinguish three sets of variables: goals, controlled variables, and uncontrolled or exogenous variables; and the purpose of the model is to arrive at decisions or decision rules concerning the value to be assigned to the controlled variables.

The second common feature is the reliance on empirical verification, as distinguished from logical consistency, as the final criterion for accepting or rejecting the model. In empirical science, the test consists of verifying the empirical implications of the model: if these implications are not verified, the model is rejected as "useless." In the case of managerial economics, the test of the model—its acceptance or rejection—is based on verifying whether the recommendations to which it has led produce an improvement in performance.

This similarity in methods and differences in objectives helps to make sense of Mr. Baumol's central contention, with which I largely agree in spite of its paradoxical appearance. According to him, there is not much that economic theory per se can offer to managerial economics, but at the same time the trained theorist may have a great deal to contribute. This last assertion is of course supported by the large list of distinguished theorists that have made significant contributions to managerial economics in the last decade. According to Mr. Baumol, the theorist's strength lies in his experience and skill in model building. I would push this point one step further. It so happens that in constructing models that will help him understand the functioning of an economy—at least of the private type—the economic theorist has generally found it useful, as a first approximation, to make use of the assumption of rational economizing behavior on the part of the participants in the economic

system. He therefore tends to devote considerable effort to figuring out how the rational maximizing (or minimizing) agent would behave in various circumstances and respond to changes. This means that the theorist is not merely skilled in the construction of models, but more specifically of models dealing with economizing behavior. But these are precisely the kinds of models that are generally useful in managerial economics. However, the economist builds his model, not to advise the agent on how he should behave, but merely because this model helps him to derive testable implications about the functioning of the economic system. For the managerial economist, the very opposite is true.

This difference of goals tends to produce significant differences between the positive and the managerial economist model. One such difference lies in the area of operationality. Because the positive economist is not concerned with giving advice, he need not bother with the question of how the rational behavior implied by his model could possibly be implemented. By contrast, operationality must be the managerial economist's prime concern: his advice is useless unless he can show how it can be implemented.

A second important difference lies in the choice as to which features of the situation are essential and must be incorporated in the model and which can be neglected. Here the positive economist is likely to concentrate on those elements which are common to many agents and to neglect what seem idiosyncratic aspects of the problem, which—he hopes—will tend to wash out under aggregation or will at worst show up as random components in his model. By contrast, these features may have to play a critical role in the managerial economist's model. It is thus not surprising that an economist's training may be a valuable asset in managerial economics, even though the theorist's model and theorems may have little direct applicability.

Consider now the prospect for useful feedbacks in the opposite direction, which forms the object of the paper by Messrs. Hitch and McKean. These authors draw for us a much more hopeful picture. As indicated earlier, I have several reservations about their reasoning and conclusions. They seem to put particular stress on the possibility that managerial economics may help to improve the "behavioral inputs" to the economist's model. By this, I take it, they mean primarily replacing the postulate of rational maximizing behavior with some more fruitful ones. For my part, I am somewhat skeptical about the chances of developing alternative postulates which are capable of broad applicability and yet are operational enough to lead to precise verifiable implications. Furthermore, such improvements seem to me more likely to come from the developments of positive microeconomic models such as those represented by the so-called "behavioral theory" of the firm than from managerial economics. Certainly in my experience as managerial economist, I have had no significant reason to doubt that firms are interested in long-run profits, although I have come to appreciate how hard it is to choose the behavior that best serves this goal.

On the other hand, I can see several other ways in which managerial economics has contributed and can further contribute to economic theory. In particular, I feel that the managerial economist's endeavor to give operational

content to maximizing behavior has helped me to get a much firmer grasp of abstract notions and concepts used by the theorist, especially in the area of production and capital theory. Also I can think of instances in which models developed for managerial purposes have helped to provide a more solid optimizing foundation for some elements of the economist model. This might be illustrated by the contributions of models developed to improve inventory management to an understanding of the transaction demand for money and of the role of inventories in the inventory cycle.

Messrs. Hitch and McKean conclude their paper with the suggestion that one important way managerial economics can contribute to economics is by increasing the efficiency of the economy! This conclusion is unconventional, but it is consistent with their notion that the central concern of economics is with the efficient allocation of resources. For my part, since my main interest is in positive economics—understanding how the system works—I can see a far more promising direction in which managerial economics can contribute to economic theory; namely, by leading economic agents to behave more nearly in accordance with the rational behavior model of the theorist. In other words, we may hope that managerial economics will contribute to increase the predictive value of economic theory, not by modifying this theory to fit the facts, but instead by modifying the facts so they will better fit the theory.

FRONTIERS IN UNCERTAINTY THEORY: THE EVIDENCE OF FUTURES MARKETS

NEW CONCEPTS CONCERNING FUTURES MARKETS AND PRICES

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1. *Introduction.* In science the frontiers that are interesting are those that are moving. When James B. Conant undertook, after World War II, to help laymen to understand science, he emphasized its dynamic aspect. Though most people tend to think of science as consisting of a body of knowledge, such a body, if it grows static, becomes dogma, antithetical in spirit to science. Science advances on the basis of continuing observations—either observations made under the conditions of controlled experiment or discriminatingly planned observations made under the conditions of nature—but the forward steps of science, according to Conant, are best marked, not by accumulation of data, but by the emergence of fruitful new concepts that arise out of scientific observation.

By way of definition, Conant said "science emerges from the other progressive activities of man to the extent that new concepts arise from experiments and observations, and the new concepts in turn lead to further experiments and observations" (*On Understanding Science,* page 24, Yale University Press, 1947). In speaking thus of experiments and observations, Conant was thinking of science in general, including both experimental sciences and those like astronomy and geology that must rely mainly or wholly on observation in nature. When the reference is to a particular science, the definition might better speak of experiments *or* observations.

A particularly noteworthy feature of this definition is that it marks progress, not in terms of theories or laws of nature, as in some earlier definitions, but in terms of concepts arising out of experiments or observations. New and fruitful concepts mark progress in science in two respects. They are, on the one hand, a major requisite for continuing scientific progress, opening up new avenues for scientifically useful observation, experiment, or analysis. And they record past advances in knowledge discriminatingly, ignoring those accumulations of factual information that are merely descriptive, or that combine description with contribution to the practical arts, while selectively summarizing such accumulations of knowledge as have been recognized as having

scientific significance. Thus the Conantian view does not break with the older one but refines it, identifying science not with all organized knowledge but with progressively accumulating knowledge that leads to what we call "understanding."

Progress in economic study of futures markets and futures prices has been marked by emergence during the last forty years of a full half-dozen new concepts that, as they gain wide acceptance, will revolutionize almost every aspect of economic thinking on futures markets. Each of these new concepts has arisen directly from the results of thoughtful, well-directed observation. Each, except the very latest of the six, has already proved a fruitful stimulus and guide to further inquiry; and the latest concept, though too recently emerged to have borne fruit, can be immediately recognized as pointing the way to new lines of inquiry.

I list the new concepts below with a parallel listing of the concepts that they partially or wholly displace.

CONCEPTS CONCERNING FUTURES MARKETS

*New Concepts**	*Displaced Concepts*
1. Open-contract concept: Futures markets serve primarily to facilitate contract holding. (1922)	Futures markets serve primarily to facilitate buying and selling. (Disproved)
2. Hedging-market concept: Futures markets depend for their existence primarily on hedging. (1934; 1953†)	Futures markets depend for their existence primarily on speculation. (Disproved)
3. Multipurpose concept of hedging: Hedging is done for a variety of different purposes and must be defined as the use of futures contracts as a temporary substitute for a merchandising contract, without specifying the purpose. (1953)	Hedging is done solely to avoid or reduce risk. (Disproved)
4. Price-of-storage concept: Differences among spot prices and futures prices for various delivery dates, for storable commodities, are themselves prices of storage, directly determined by pertinent supply and demand conditions; hence prices for near and distant dates of delivery are about equally affected by distant expectations. (1933; 1949)	Price differences for different dates of delivery often result from an absence of influence of distant expectations on prices for near deliveries. (True in special circumstances, but proved usually untrue.)
5. Concept of reliably anticipatory prices: Futures prices tend to be highly reliable estimates of what should be expected on the basis of contemporarily available information concerning pres-	Futures prices are highly unreliable estimates of what should be expected on the basis of existing information; their changes are largely unwarranted. (A wholly un-

* Dates shown in parenthesis are years in which the concepts may be said to have emerged. Where two dates are shown, the earlier one is the date of publication of evidence recognized as challenging the older concept; the later date, that of first known publication of at least the substance of the new concept.

† A statement of the new concept was first prepared for publication by H. S. Irwin about 1946, but Irwin eventually found it necessary to publish privately, in 1954. Thus it happened that another author, benefiting from knowledge of Irwin's work, gave first published expression to the new concept.

ent and probable future demand and supply; price changes are mainly appropriate market responses to changes in information on supply and demand prospects. (1934; 1949)

proved inference; accumulating evidence mainly supports the new concept.)

6. Market-balance concept:

A significant tendency for futures prices to rise during the life of each future is not uniformly present in futures markets, and when it exists is to be attributed chiefly to lack of balance in the market. (1948; 1960)

There is a tendency for futures prices to rise during the life of each future ("normal backwardation"), which is attributable to aversion toward risk-taking. (Concept shown to have rested largely on inadequate and misleading evidence.)

(What follows is a highly abbreviated version of the remaining sections, which will be published in full elsewhere.)

2. First of the six new concepts to emerge, and a necessary precursor of much that was to follow, was the concept that the business of futures markets should be measured primarily in terms of volume of contracts outstanding—so-called "open contracts." This was a revolutionary concept, carrying with it recognition that the traditional function of markets, transfer of ownership, was not the primary function of a futures market. Credit for it belongs chiefly, I think, to J. W. T. Duvel, of the Grain Futures Administration, who was shortly to become the first administrator of that federal agency, and during his long occupancy of that position gave research a prominent place in the activity of that regulatory body.

3. Though economists have often imagined that futures markets can exist purely on the basis of speculative participation, that is not true; hedging is necessary for the existence of a futures market and the size of the market is determined primarily by the amount of hedging. These facts were brought to light primarily by H. S. Irwin. A recent study has revealed that speculation enters and leaves a futures market in extraordinarily close and sensitive, week-to-week response to changes in the amount of hedging.

4. Risk-taking is a major source of profit in business; hence hedging is usually done for one or more reasons other than that of risk avoidance. The traditional economic view that the sole function of hedging is risk-shifting has been a source of serious error; it was, for example, responsible for a misinterpretation that proved crucial in leading to enactment in 1958 of a federal law prohibiting futures trading in onions.

5. The price-of-storage concept is significant chiefly because it reveals why spot (cash) prices and prices of old-crop futures respond sensitively to changes in new-crop expectations even when the old-crop price is far above the expected price for the new crop.

6. Though all of the six concepts discussed here had their basis partly or wholly in quantitative economic investigation, only one has depended heavily on mathematically sophisticated econometric work.

7. Whereas the "fluctuations" of futures prices, like those of stock prices, have usually been regarded as showing much random variation, they exhibit instead a close approximation to pure random walk, which gives evidence that the prices are to a high degree reliably anticipatory. Expectations, it must be recognized, may be highly reliable interpretations of what should be expected in the light of currently available information even though fulfillment of the expectations be quite imperfect.

8. The concept that risk-bearing commands a reward, applied at several related points in the theory of futures markets, has served poorly to account for observed phenomena of the markets. One of its applications led J. M. Keynes to advance the concept of "normal backwardation," but the observed tendencies toward backwardation vary widely according to circumstances, as shown by R. W. Gray, in a manner not reasonably explainable on the basis of differences in risk premium.[1]

9. The foregoing review of a half-dozen new economic concepts that have emerged from observation, if not experiment, and have been fruitful of further observation, in the manner described by Conant's dynamic definition of science, leads to inquiring why Conant and many other scientists have thought that economics has little or none of the character of a science.

[1] The more familiar recent argument against Keynes's theory has denied the existence of statistical evidence of a tendency toward backwardation; my paper, as originally presented, failed to make it clear that Gray's challenge to Keynes's theory rests on the quite different ground stated explicity above.

SYSTEMATIC AND RANDOM ELEMENTS IN SHORT-TERM PRICE MOVEMENTS*

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I. *Are Price Changes Random?*

Following Holbrook Working's precept in the preceding paper let me start by discussing the empirical observations that prompted the theoretical analysis in the second part of my article. These observations fall under two headings: the first concerning the results of certain trading policies in wheat and corn futures and the second some aspects of price changes in spot and futures prices of cotton.

The question whether prices in speculative markets move randomly has so far been answered mostly in the affirmative [3, 4]. It should be realized, however, that randomness can only be defined negatively; namely, as the absence of any systematic pattern. A particular test can detect only a particular pattern or class of patterns, and complete randomness can therefore only be disproved, not proved. The results just mentioned do show that any systematic pattern in price changes is not likely to be obvious or simple.

Indeed, we might be tempted to argue that if any pattern whatsoever persisted over a long time interval, it would be discovered by traders, who would proceed to use it to their advantage and thus destroy either the pattern or the market. This argument overlooks the fact that commodity price developments are watched by relatively few traders, most of them quite set in their ways; even in the most active futures markets the volume of serious research by participants seems to be quite small. It is therefore possible that systematic patterns will remain largely unknown for a very long time.

The particular pattern of which I will present some evidence results from the use of so-called "stop orders." An order to buy (or sell) may be of three varieties: it may be "at the market" (buy at any price), limited (buy if the price is no more than a given value), or "on stop" (buy if the price reaches a given value from below). The reasons for using stop orders and their effect on price behavior are discussed in the next section. Here I deal only with the profitability of stop orders as an indication of nonrandomness.

Consider a trader who each year on the same date (say, February 1) buys a certain futures contract, say, September wheat, and who sells the contract on June 1. His profit or loss per bushel will be equal to the change in the September price between February 1 and June 1, less commission. Now suppose that rather than always maintain his position until June 1, he is prepared to sell at an earlier date, depending on the course of prices after February 1. More particularly, suppose that he tries to limit his losses by selling whenever the price falls a fixed percentage (say, 5 per cent) below the initial price; this means that he places a stop order to sell at 95 per cent of his purchase price.

If price changes were random, such a policy would not have any effect on his average profits over a number of years. A price fall of 5 per cent or any other figure would not affect the distribution of subsequent price changes and would therefore not provide any information according to which the trader should revise his plans. (It is true that this policy safeguards the trader against large losses, but we are only concerned with average profit at this point.) If, on the other hand, a price fall is on balance likely to lead to a further fall, timely liquidation will reduce losses without affecting profits and hence will increase average profit. In that case, everything will depend on the timeliness, that is, on the percentage, at which the stop order becomes effective. If the stop percentage is too small, insignificant fluctuations ("noise") will lead to premature liquidation and the abandonment of profit opportunities. If it is too large, the losses suffered before liquidation may be too heavy to be balanced by profits.

In an attempt to clarify these matters the profitability of stop-order policies for varying stop percentages has been calculated for wheat and corn futures during the periods October 1, 1921, to October 1, 1939, and February 1, 1947, to October 1, 1956. Both long and short positions were considered. It was assumed that a trader bought (or sold) the May future on October 1, the September future on February 1, and the December future on June 1, each being liquidated after four months unless a stop order became effective at an earlier date. All positions were initiated at the closing price of the opening day, but intraday highs and lows were taken to release stop orders; liquidation was either at the stop price or at the closing price of the final day. Tables 1 and 2 show the financial results rounded off to cents per bushel over the period as a whole. A stop percentage of 100 means that no stop order was used, so that the position always remained open for the full four months. A stop percentage of zero means that the position was liquidated as soon as the price fell below (in case of a long position) or rose above (in case of a short position) the initial price. Commissions, which are the same for every policy, were not taken into account.

TABLE 1

Trading Results in Wheat Futures (1921–39, 1947–56) for Various Stop Percentages, in Cents per Bushel

Stop Per- centage	Long Positions				Short Positions			
	May	Septem- ber	Decem- ber	Com- bined	May	Septem- ber	Decem- ber	Com- bined
100...	+127	− 99	+ 59	+ 87	−127	+ 99	−59	− 87
20...	+104	−126	+ 55	+ 33	−100	+112	−16	− 4
15...	+ 96	−130	+ 69	+ 35	− 83	+ 73	−24	− 34
10...	+132	−123	+ 90	+ 99	− 68	+ 90	+10	+ 32
7½..	+135	− 74	+126	+187	− 55	+ 57	+27	+ 29
5...	+122	− 47	+140	+215	− 34	+ 83	+79	+128
4...	+133	− 15	+ 65	+183	− 19	+ 94	+53	+128
3...	+116	− 5	+ 78	+189	− 19	+ 94	+70	+146
2...	+ 42	− 4	+ 84	+122	− 24	+ 71	+30	+ 77
1...	− 2	− 27	+ 73	+ 44	− 27	+ 60	−11	+ 22
0...	+ 18	0	+ 27	+ 45	0	0	0	0

These two tables call for several comments. In the first place, it will be noticed that without the use of stop orders (that is, for a stop percentage of 100), long positions were profitable in the May and December futures for both wheat and corn, and unprofitable in the September future; a consistently long position in all three futures was also profitable. This is remarkable because in the two periods of observation spot wheat and corn prices fell on the whole (about 28 cents a bushel for both wheat and corn). The rise in the May and December futures may be attributed partly to seasonality (which also accounts for most of the fall in the September future) and partly to "normal backwardation." Both these phenomena imply that price changes are not purely random but follow certain longer run trends (longer, that is, than the

TABLE 2

Trading Results in Corn Futures (1922–39, 1947–56) for Various Stop Percentages in Cents per Bushel

Stop Per- centage	Long Positions				Short Positions			
	May	Septem- ber	Decem- ber	Com- bined	May	Septem- ber	Decem- ber	Com- bined
100...	+79	− 83	+112	+108	− 79	+ 83	−112	−108
20...	+75	−114	+108	+ 69	−111	+ 94	− 36	− 54
15...	+95	− 94	+119	+120	− 80	+ 91	− 48	− 37
10...	+83	− 73	+141	+151	− 37	+107	− 47	+ 23
7½..	+57	− 97	+151	+111	− 45	+103	− 27	+ 31
5...	+21	− 47	+127	+101	− 23	+104	− 19	+ 62
4...	+35	− 34	+125	+126	− 28	+117	− 56	+ 33
3...	+36	− 11	+139	+164	− 12	+115	− 68	+ 35
2...	+ 9	− 17	+156	+148	− 10	+105	− 53	+ 42
1...	+29	− 24	+170	+175	− 9	+ 66	− 27	+ 32
0...	+29	+ 2	+ 70	+100	0	0	0	0

daily fluctuations considered here). This does not greatly affect the problem of randomness in the short run.

The results for a stop percentage of zero are also interesting. In a small number of cases the initial price turned out to be lower than any subsequent price in the four-month holding period. In those cases there was consequently a profit, but there were no offsetting losses under the assumptions made here. These assumptions, however, may give a wrong impression of the profitability of a zero-stop policy: not only have commissions been ignored, but in reality it would not always be possible to liquidate a position at the stop price. Frequently a somewhat worse price (lower on stop-sell or higher on stop-buy orders) will prevail because of market imperfections or time lags.

The profitable instances just mentioned are fairly rare (three in May wheat, one in December wheat, two in May corn, one in September corn, and three in December corn out of twenty-seven or twenty-eight possibilities on the long side). Recent theoretical work [1, Chapter III] has shown that in randomly changing series such instances are not as rare as had previously been thought. Thus, if over the four-month period the price had changed a thousand times in either direction, a zero-stop policy would have paid off, whether on the short or on the long side, once in about thirty-five instances. The observed frequency is about twice as large and it is also strange that all observed instances are on the long side. Whether these peculiarities are by themselves sufficient to cast doubt on the hypothesis of randomness is not clear.

The results with stop percentages between zero and 100, however, provide somewhat more definite evidence against randomness. In every future, whether long or short, it is possible to do better by using some stop percentage than by using none (that is, by using 100 per cent). The improvement is not always very large and, on the whole, stop orders, as used in this analysis, seem to be more effective in reducing losses than in increasing profits. In no case can a stop policy with a fixed stop percentage turn an unprofitable operation (such as buying the September future) into a profitable one.

There seem to be regular patterns in the results for each position with different stop percentages, though they are not very marked. In the case of a long position in May corn, for example, fairly large stop percentages seem to give better results than small ones, but the opposite is true for December corn. The results for long positions in May and December wheat and for a short position in September corn are somewhat more clear cut in favor of moderate stop percentages. The irregularity of these patterns is no doubt due in large part to the relatively small number of observations.

The statistical significance of the results for stop percentages between zero and 100 is hard to assess in the absence of a more developed theory of randomly changing series. The influence of seasonality and normal backwardation noted previously is also a complicating factor. Nevertheless, I feel that Tables 1 and 2 indicate the existence of patterns of price behavior that would not be present if price changes were random.

In support of this conclusion it may further be pointed out that the assumptions made so far bring out no more than a part of the potential of stop-order policies. In the above calculations the starting date of each transaction was fixed, but if stop-order policies are at all effective, they could be used profitably to determine not only when to end but also when to start a transaction. Moreover, the policy could presumably be made more effective by letting the stop percentage refer not to the initial price but to the highest or lowest price in some recent interval. These refinements remain to be investigated.

II. *Analysis of Daily Cotton Prices*

Space limitations prevent a full discussion of the other set of data mentioned earlier, and only a few remarks must suffice. The data used are daily closing prices of spot cotton and the six nearest futures contracts from October, 1944, to July, 1958, excluding the period from January 27, 1951, to the expiration of the July, 1951, future which was affected by government price ceilings. The original purpose of the analysis was to estimate the parameters in a linear stochastic process by which the data might be represented, but the results obtained so far have made it advisable to reconsider this aim. Some of these results are:

The distribution of day-to-day changes in the logarithms of prices does not conform to the normal curve. It is not significantly skew, but highly leptokurtic (that is, there are more very large and more very small deviations than in a normal distribution with the same mean and variance). This phenomenon had also been noticed in similar data by Kendall [3]. It complicates the application of the available methods of time-series analysis, which are none too satisfactory even for the normal case.

The variance of price changes does not seem to be constant over time; thus it is about four times as large for the first half of the period (before the interruption in 1951) as for the second, and it also appears to be quite variable within shorter intervals. Very large deviations, in fact, seem to come in bunches. The leptokurticity mentioned above may be related to the changing variance.

The intercorrelations between spot and futures prices are higher in the first half of the period than in the second. Combined with the difference in the two variances this agrees with Working's observation [5] that hedging is relatively more effective as a protection against large price changes than against small price changes.

The correlograms of the seven price series, which have been computed for lags up to 120, do not show any obvious pattern. More particularly they reveal little positive serial correlation, which might otherwise account for the profitability of stop percentages. It may therefore be suspected that a nonlinear stochastic process is at work, the detection of which requires different techniques of analysis. Work along these lines, especially the calculation of transition matrices, is now in progress.

III. *Theoretical Discussion*

The purpose of the following is to call attention to some theoretical peculiarities of short-term price movements in speculative markets and particularly to those peculiarities that may lead to the phenomena observed in the data. In the "market period" the price is determined by the physically available stocks (which are zero in the case of commodity futures markets) and the excess demand curve for these stocks. The latter curve is subject to shifts, which in the very short run contain a preponderant stochastic element. The price is given by those intersections of the vertical axis and the excess demand curve at which the latter has a negative slope; intersections at positive slope are irrelevant because any plausible *tâtonnement* process will overshoot them. Nevertheless, the possibility of upward sloping segments of the demand curve is of great importance for the explanation of short-term price movements.

To see this, consider Figure 1, where the excess demand curve has one "positive" and two "negative" intersections. If the previous price was somewhere near A or near C the new price (corresponding to the demand curve shown) will not be very different from the old one. But if the old price happened to be near B, a much larger price change is necessary to reach equilibrium. Upward sloping segments of the excess demand curve may therefore lead to disproportionately large price changes.

There are two reasons for the occurrence of upward sloping segments in actual speculative markets. The first is well known; it is so-called "movement trading," especially the "explosive" variety in which a price rise leads traders to act as if they expect a further price rise, and similarly for a price fall. If price changes were random, how-

ever, there would be no profit in such behavior, nor would it become profitable because "everybody is doing it."[1] Explosive movement trading can reinforce any instability that is already present, but it cannot be the origin of instability. For the origin we have to look elsewhere.
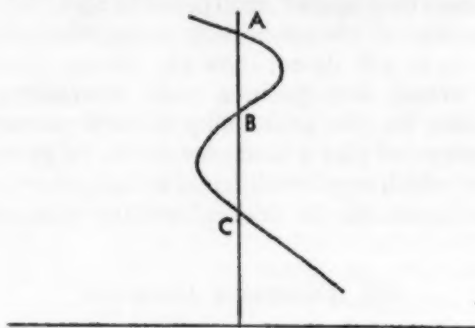
FIGURE 1

The second cause of upward sloping segments of the excess demand curve is familiar enough to the economic theorist but has rarely been applied to speculative markets. I refer to the Griffen paradox, which results if the income effect of a price change outweighs the substitution effect. This paradox has never been convincingly detected in data on consumption, but its chances of occurrence are much greater in speculation. Since in the present context we are concerned with stocks rather than flows, the income effect becomes a wealth effect.

The wealth effect is likely to be of importance to many speculators because futures are normally traded on very low margin. The equity which a speculator has to supply is rarely more than 10 per cent of the money value of the contracts he buys or sells; 5 per cent is a typical figure. This, however, is only the initial margin. If the price changes against the speculator, he has to provide a variation margin equal to the full change in the value of the contract unless he prefers to liquidate. Thus a speculative buyer of a cotton futures contract (50,000 pounds, currently worth some $16,500) is required to put up an initial margin of $750. If the price falls by a cent and a half, his initial margin will be wiped out; in fact he will be asked for variation margin well before this point. Because of this high gearing, the effect of price

[1] Another variety may be called "cyclical" movement trading; here a price rise would lead traders to expect a corrective fall later on. The explosive movement trader acts as if prices go up and up (or down and down); his cyclical counterpart as if they go up and down. The former will use stop orders so as to "cut his losses and let his profits run," while the latter will "take profits." Evidently explosive and cyclical movement trading will tend to offset each other.

changes on speculators' wealth, and especially on liquid assets, may be considerable; so they often protect themselves by stop orders. Whether such orders are used or not, if price changes are sharp and sudden, they will sometimes force even the strongest traders to liquidate and thus reinforce the original price change. As has been argued elsewhere [2], trading on low margin is an essential feature of viable futures markets.

The price change just considered was unfavorable. There is also a wealth effect associated with a favorable price change; namely, the increase in the equity of those traders who are already in the market. The paper profits may be used as initial margin for an extended position—a practice known as pyramiding. The formal symmetry between forced liquidation and pyramiding should not be carried too far, however; the element of constraint is absent in pyramiding, which may perhaps be more accurately regarded as a form of explosive movement trading.

The wealth effect does not concern those who are outside the market when the price changes. Their reaction to the price change, if it goes in the normal direction of a substitution effect, may offset the wealth effect. It cannot always be counted upon to do so, and if it does not, the excess demand curve will have an upward sloping segment.

It will be seen that quite a few conditions have to be fulfilled before the disproportionate price changes discussed in connection with Figure 1 can come about. The wealth effect on existing positions has to outweigh the substitution effect, or there has to be substantial explosive movement trading (or both); furthermore the upward sloping segment has to intersect the vertical axis and the previous price has to be near this intersection. This kind of instability is therefore not likely to arise frequently.

A disturbing conclusion may nevertheless be drawn from the above factual and theoretical analysis, sketchy and tentative though it is. If there is an original element of instability it will be profitable, at least to the quick witted, to reinforce it by stop-order policies of the kind discussed in Section I.[2] Such destabilizing policies may in turn undermine the whole process of price determination. Should we rely on the well-founded expectation that few speculators take economists seri-

[2] Actually, stop orders as such are a symptom rather than a cause of destabilizing market behavior. Essential is the willingness of traders to sell when the price goes below a certain level, or to buy when it goes above it. Stop orders are merely a convenient device for implementing this type of behavior. Little or nothing would be gained, consequently, by prohibiting stop orders, as was proposed many years ago in the Federal Trade Commission's *Report on the Grain Trade*. The purpose now served by stop orders could be achieved with slightly more trouble by means of market orders.

ously? Or is there some unknown feature of competitive markets which would protect them even against this abuse?

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COMMON ELEMENTS IN FUTURES MARKETS FOR COMMODITIES AND BONDS*

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The reasons for the relative paucity of well-developed futures markets are far from trivial. They lie at the heart of many of the key questions about the behavior of a real economic system over time: attitudes toward risk, accuracy of foresight, the imperfections of markets. The more limited purpose of this paper is to summarize some of the results of studies, part of a larger investigation, into two different kinds of well-developed futures markets with an eye toward establishing certain of their common elements that may facilitate a proper evaluation of their role.

I

The key role of a futures market is in facilitating equilibrium across time. If all goods were instantaneously perishable, the future would be irrelevant to equilibrium in the present. On the other hand, the opportunity for storing goods, whether in the form of commodity inventories or "congealed" as plant and equipment, opens up a potential for tinkering with present and future equilibrium. One of the most interesting aspects of this opportunity for intertemporal adjustment is its asymmetry: the fact that currently-produced goods can be held for future consumption, but, in general, it is not possible to live off the bounty of future generations.

If we expected the price of a storage good to rise in the future by an amount greater than the costs of storing that good, the profit motive would give us an incentive to add to our inventory of that commodity. In a world of static certainty, the price of a storable commodity could not increase over any period of time by more than the marginal costs of storing the good over the interval. If prices rose enough to cover such costs of storage, we could be sure that inventories would be held, but prices could rise by lesser amounts without resulting in storage. More important, perhaps, prices could be expected to fall in the future without any effect on the current equilibrium position. Once inventories have fallen to zero, our ability to adjust to future bounty is limited to our willingness to restrict present consumption for future income.

If, therefore, we were to assume the existence of perfect certainty

about the future and that warehouse space could be produced indef-
initely at constant cost, the level of inventories currently held would
be an indicator of the level of expected price change, though an im-
perfect one. If the level of inventories was greater than zero, an ob-
server could, with unfailing accuracy, predict that the instantaneous
rate of price change was equal to the instantaneous rate of accretion
of storage charges. If inventories were not held, however, we could
not specify whether prices would rise or fall, only that they could not
rise by as much as storage costs.

The case of perfect certainty is not a very interesting one. If we
know what all future prices are going to be without the mediation of
markets, then we need to look no further to ascertain reasons for their
nonexistence. If, on the other hand, we permit the introduction of
uncertainty, this simple supply curve of storage is no longer sufficient.

The introduction of uncertainty implies the introduction of risk and
it poses all of the problems of evaluating individuals' attitudes for
risk. Partly because exploring all of these possibilities would take us
too far afield in a paper of this nature and partly out of empirical con-
siderations, we will assume that everyone is Tobinesque. For any given
level of return, people prefer less risk to more risk and for any given
level of risk, they prefer more return to less. We will also assume that
there is a one-to-one correspondence between risk and the statistical
concept of variance.

As a manufacturer increases his holdings of inventories in an uncer-
tain world, he increases the risk he faces. As the level of inventory
rises, it comprises an increasing proportion of his total assets so that
forecasting error of any given absolute magnitude would lead to a
greater effect upon the level of his over-all wealth or income. Accord-
ing to our model of economic behavior, therefore, the rational economic
man will demand an extra risk premium above and beyond the marginal
out-of-pocket storage charges required to induce him to carry addi-
tional units of inventory, and this risk premium will increase as the
level of inventory holdings increases. This means that large inventories
will not be held unless the expected price increase is greater than that
which would be required to cover cash storage costs by an amount large
enough to offset the additional risk involved.

The risk that prices will not rise as expected is not, however, the
only inventory risk faced by a manufacturer in a real world. There are
also uncertainties about the delivery of raw materials on schedule and
about the level of demand for final product. If there are any important
fixed costs in his production process in the short run, there is the risk
of real financial loss if the process is interrupted by the lack of in-
ventory of the right type or quantity. Furthermore, he knows that the

likelihood of interruption varies inversely with the level of inventories that he holds. Thus, even when inventories are not expected to produce benefits in the form of price appreciation, they may show advantages from the point of view of production costs. What this means is that under certain conditions inventories will be held even if the expected price increase will not cover storage costs alone. In extreme circumstances, inventories may be held when prices are expected to decline

Expected Price
Less Present Price

Risk Premium

Warehousing
Costs

O

Inventories

Convenience
Yield

(a)

Expected Price
Less Present Price

Risk
Premium

Inventories

Supply Curve of Unhedged
Inventory

Supply Curve of Hedged Inventory

(b)
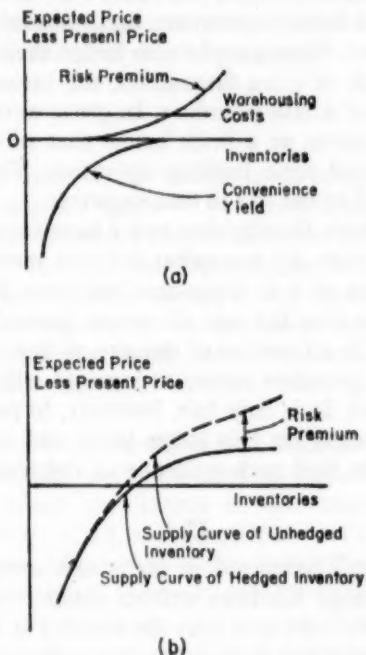
FIGURE 1

substantially. This will happen if the amount of inventories is sufficiently low and the costs of interruption to production are sufficiently great.

Empirical studies by Working, Brennan, and Telser have all tended to confirm the theoretical arguments we have made so far. The over-all shape of the supply curve of storage for a wide range of commodities has fallen into the pattern shown in Figure 1b. On this point, there seems to be widespread agreement. There is much less agreement on the nature, or even the existence of, risk premia of the kind indicated in Figure 1a, but the work done by Brennen and Houthakker and other studies of my own are at least compatible with such shapes, and in

addition, we come to similar quantitative conclusions about the parameters. While I have rather strong convictions about the validity of these conclusions, it is only fair to point out that some uncertainty remains.

Since we have no way of determining *ex ante* expectations, the techniques for estimating risk premia are necessarily indirect. One technique is to assume that all parties expected the prices that actually came to pass, and evaluate their decisions on this basis. Hedgers are usually net sellers of futures contracts, and speculators are usually net purchasers of futures. Since people who hedge their inventories eliminate most of the risk of price fluctuation, the futures prices at which they hedge are net of a risk premium. In other words, hedgers should be willing to sell futures at a price below that which they expect to prevail when they end their hedging operation. The size of the risk premium is measured by the rise in futures prices.

By this method, both Houthakker and I have found trends of about 5 to 8 per cent per year. By somewhat different methods, Brennan has estimated risk premia of 6 to 8 per cent per year. In addition, I have found some evidence that the rate of return demanded by speculators in wheat and cotton is a function of the size of their positions. That is, the size of the risk premium seems to increase with the net quantity of inventories hedged. It is only fair, however, to point out that there is still some controversy on this latter point and that earlier studies by Working failed to find such evidence of risk premia in the wheat market.

II

Very little of what I have said so far is new except perhaps in the sense that much of what has been written about futures markets is so scattered and esoteric that you may be hearing it for the first time. But all of this material has dealt with the markets for physical commodities, partly because the main body of markets explicitly referred to as futures markets does deal in such commodities. On the other hand, we all know from our textbooks that the existence of markets for bonds of identical quality but of varying maturity comprises in effect a set of quotations for expected interest rates in the future. If the hypotheses and empirical conclusions we have formulated for commodity markets are possessed of any general validity, the same concepts ought to apply to the futures market for debt implied in the term structure of rates. Similarity of behavior would be particularly interesting in view of the fact that debt markets extend for forty years or more, while commodity contracts rarely extend beyond eighteen-month maturities.

No rational investor in a riskless society would accept a yield of

R_2 per cent on a bond of two-year maturity at the present time if he knew with certainty that he could earn a higher rate of return by purchasing a one-year bond today and reinvesting the proceeds at the end of the year in a similar bond. Similarly, he would not buy the one-year bond if he knew that when time came to reinvest the yield of the new one-year bonds would be so low that he could have earned more income over the two-year period by purchasing the two-year bond at the beginning of the period. As a result of this implicit arbitrage, the existence of current yields of two-year and one-year bonds implies a prediction that one-year bonds will next year be yielding

$$R_{12} = \frac{(1 + R_2)^2}{1 + R_1} - 1$$

If there were any tendency for the price of one-year bonds to fall, individuals would have the incentive to buy such bonds and add them to their portfolio (read inventory) up to the point where their opportunity cost of funds for "one-year-starting-one-year-from-now" was equal to R_{12}. This opportunity cost of funds is very much like the warehouse costs to which we referred in the discussion of commodity markets. No one will hold two-year bonds unless the implied rate of interest on one-year money one year from now is at least as great as that cost of funds.

Such is the case with perfect certainty. Like manufacturers, however, bankers face risks, but unlike the case of manufacturers, there is still much confusion about the nature of the important portfolio risks faced by banks. First of all, we must make very clear that the banking system is not harmed by a rise in the level of interest rates. It is unfortunate that this truism, restated analytically by Samuelson fifteen years ago, still is incompletely understood. Few people will fail to realize that in the long run, a rise in interest rates will increase the amount of income that a bank will earn on a given volume of loans. But few people will be intuitively convinced that even in the short run it will be benefited, unless the bank expects imminent liquidation or unless its portfolio is entirely consols. It is true the market value of its portfolio will fall, but it will almost immediately start earning more money on current account than it had before. The bank can only suffer by the amount that deposits are lost (on a net basis) faster than portions of the portfolio reach maturity, after allowing for the benefits of the higher yields on the portion that is not withdrawn. A bank that shows steadily rising deposits cannot lose at all.

A bank could fail if the decline in its market value produced a run. In today's real world, that is not very likely, but it is a possibility

against which managers must guard and one which induces them to maintain somewhat more liquidity than they otherwise would. An even better reason for holdings of liquid assets by banks, however, is likely to be the superiority of those assets in making it possible to take advantage of the banks' comparative advantage in the market for loans. When loan demand is particularly strong and offers a particularly attractive opportunity for employment of funds, the bank is best equipped to take advantage of the situation if its securities are readily marketable with minimum capital loss. This is particularly true because the less liquid is the banking system, the more likely it is that such attractive opportunities for employing funds will actually arise. This is because remunerative rates and the possibility of attracting new customers are positively associated with general bank illiquidity. Like the manufacturer's behavior when inventories are small, a banker may be willing to hold such liquid assets even when he could earn a greater return by investing in longer maturities in order to avoid losses or take advantage of possible gains in the lending section of his business. This is analogous to the convenience yield of the manufacturer, and, as in that case, it is a price which is likely to be paid only in the relatively rare case when inventories of liquidity are exceptionally low. There is no reason to suppose that this will be the predominant case in the banking system.

The main purpose of the assets of the banking system is to earn money—customarily in the form of current income, not capital gains—and as a rule banks succeed in this objective. Furthermore, the variability of these earnings represents the major risk to the success of the bank, and fluctuation in the level of interest rates represents the major influence on the variability of the earnings. The obverse side of the rule that long-term bonds tend to fluctuate in value more than short-term bonds is that long-term bonds fluctuate less in yield than short-term bonds. For an investor seeking to minimize the risk of fluctuation in income, long-term bonds possess clear-cut advantages over shorter maturities. While this is generally clearly understood in the extreme case of insurance companies, it is less clearly seen that the same motivations apply to banks within the generally shorter range of maturities in which they choose to deal. Thus, when the term structure of rates forecast an expected yield for "one-year bonds next year" less than the bank's opportunity cost of funds, the bank would have the incentive to use funds to buy one-year maturities rather than longer bonds. As its stock of shorter maturities grew, however, it would face an increasing risk to earnings in the event of error in its expectations. In making marginal additions to its portfolio, therefore, the bank would have to add to its opportunity cost of funds a growing premium for the risk

involved in carrying the additional level of inventories. This premium is analogous to the risk premium involved in carrying physical inventories.

In general, the supply curve of liquidity in the banking system is perfectly analogous to the supply curve for the storage of physical commodities. There is, however, no necessary reason for these three factors to combine in quite the same way, and, in fact, there are some interesting differences in their behavior.

Since it was not feasible to make a detailed analysis of banks' holdings of bonds by maturity, the empirical calculations were based on the assumption that banks' inventory policies were rational. If this is so, assets of all maturities are perfect substitutes at the margin. And we can assume that the marginal return to the bank from holding a three-year bond is identical with that of a bill and this yield is exactly equal to the marginal yield on the holdings of cash. "Free reserves"—excess bank reserves minus borrowings from Federal Reserve banks—were then taken as the measure of banks' liquid asset inventory. The rate of return from holding these inventories—the expected change in yields reflected in the term structure—was measured by the difference in yield between the Federal Reserve Board's index of yields on 3 to 5 year securities and the corresponding index for 9 to 12 month securities. Unlike the case of the physical commodity supply curves, there was no discernible tendency for the curve to differ from a straight line, although this does not rule out the possibility that there is such an effect which would show up for levels of free reserves outside the range experienced in the 1953-59 period for which the line was derived.

Does the relationship I have calculated really measure what it purports to measure? First of all, it is clear that the correlation is not due to the fact that increases in free reserves are associated with declining interest rates, and "short-term rates always fall more than long-term rates." The correlation between the yield differences and the level of yields is not as good as the relation we have measured. The same is true for the correlation of reserves with the level of yields. The monetary authorities do not predetermine the banking system's desirable level of reserves independently of the differential between rates. Banks do not have to sell bills to the Fed and will not do so unless the "price is right." They will only hold the cash if they feel that the structure of yields is favorable to them. However, the most convincing argument for the validity of the hypothesis is its power in explaining the dynamic behavior of yields and differences between yields—a subject to which we will turn in the final section.

Before turning to our empirical results on the size of the risk premia, we must deal with another illusion about the structure of rates: the

belief that a short-term rate below the long-term rate necessarily implies "liquidity preference." A preference for liquidity means that investors would be willing to accept a lower yield resulting from staying invested in a short-term security for *n* years than they could get from investing in a security of *n*-year maturity. If short-term rates are below the *n*-year rate and stay that way for the next *n* years, the "normal" rate structure indeed implies liquidity preference. But it could equally well imply expectations of rising short-term interest rates and is quite compatible with liquidity aversion as well. In point of fact, we know that, historically, the widest declines of short-term rates below long-term rates have usually preceded the sharpest rises in the short-term rate.

Without a knowledge of individuals' expectations, it is difficult to determine whether they were willing to pay premiums to avoid certain types of risks, but we can test their behavior on the hypothesis that they expected what in fact happened. In short, we can compare for every week of the period the results of the two strategies: (1) buy a four-year bond or (2) buy a one-year bond and reinvest every year for four years. On the average during the period, it would have paid a speculator to have purchased the four-year bond every time free reserves dropped below the 100 million dollar level, and to have switched to the second alternative every time free reserves rose above that level. The premium yield of the longer term bond was significantly correlated with the level of liquidity. Most strikingly, every case of an "inverted" term structure of rates, i.e., when short-term rates were above long-term, proved to be a case when the liquidity premium was quite large, while the cases of exceptionally great discounts were uniformly periods when there was no premium or liquidity. Furthermore, this relationship cannot be ascribed to the business cycle, since the four-year period covers at least one full cycle at all times.

III

The final common element that I have found in both futures markets —evidence of speculative foresight—also provides the most convincing evidence of the relevance of the foregoing models. In order to analyze the dynamic properties of speculative and hedging behavior, the residuals of the supply curve of storage were subjected to closer attention. Any deviation from the curve implies returns to storage which were either more or less than the normal amounts required. By inference, these deviations implied that speculators were sufficiently enthusiastic about the potentials for price improvement to reduce the risk premium demanded of hedger or were so pessimistic about price changes as to demand exceptionally large premia. If speculators' forecasts were ac-

curate—and here we are talking about the net verdict of new money entering the market as of a moment of time and not total speculative positions—we would expect absolute prices to rise when returns were exceptionally high relative to inventories and to fall when the situation was reversed.

This is not a simple point and it bears restating. In equilibrium, a merchant's holdings of inventory imply an expectation of price change net of risk premium which can be determined from the supply curve of storage. Also, at equilibrium, the price at which a speculator assumes the opposite side of a hedged position differs from the expected price by the amount of the risk premium. Now, assume that the speculator suddenly perceives an unexpectedly large price change in the offing. He takes advantage of this by buying futures. The immediate effect of his purchases is to raise futures prices relative to the spot price. As a result, the return to storage, given current inventories, becomes exceptionally attractive, encouraging increases in inventory and hedging. If speculators' expectations are correct, such exceptionally attractive returns to storage will be followed by a price rise. If hedgers' expectations are correct, the increased hedging will restore the equilibrium relationship. The reverse argument also applies.

In the case of the government securities model, the results are striking. In 63 per cent of the cases, a deviation of any size resulted in the expected behavior. In cases of deviations greater than 10 basis points (a basis point is one hundredth of one per cent) of yield, 80 per cent of the cases followed the expected behavior. Finally, since errors proved to be less costly than successes, losses implied by the model proved to be only 6 per cent of gains. In addition, the supply curve residuals gave similar predictions when extended to a period not covered by the original data. The appended Figure 2 shows the results of the model in predicting price movements of the 3 per cent Treasury bonds of 1995 during 1960. Extensive work with the commodity markets indicates similar results, except that the predictive accuracy of the residuals was greater in the case of commodities. For example, over a twelve-year period, 1948-60, inclusive, buying rubber futures when the actual returns moved above "normal" and selling when the actual moved below normal proved to be profitable in 85 per cent of the cases. Figure 3 gives the data for 1955. A similar model gives equally good results for wool. On the other hand, the egg market does not seem capable of characterization in this manner. At least part of the inferior behavior of the bond is undoubtedly attributable to the fact that it ignores the expectations of nonbank, nonspeculative holders of liquidity.

There is much work to be done in this area and few of the observations in the paper have any claim to definitiveness. However, all of this
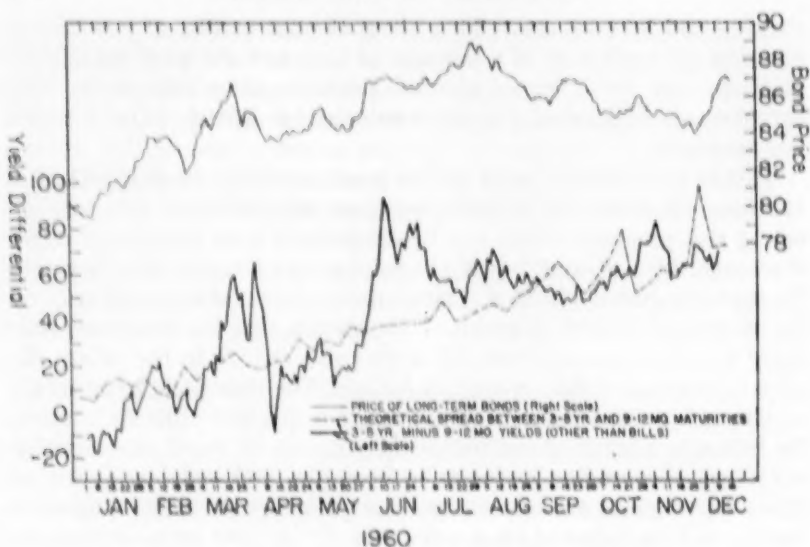
FIGURE 2
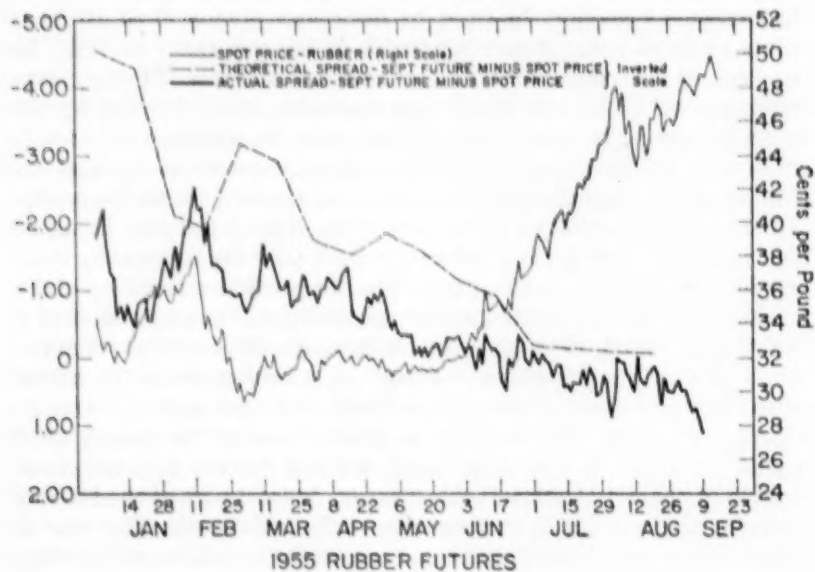
FIGURE 3

work is at least consistent with rational behavior in the face of uncertainty. The evidence is that speculators demand, and receive, risk premia for their efforts in futures markets. In return for these premia, they seem to provide a reasonably accurate forecast of short-run changes in supply and demand conditions in the markets in which they operate.

DISCUSSION

Rᴜᴛʜ P. Mᴀᴄᴋ: Mr. Working presents a case history in what he has named the economics of the empirically based concept—"a branch of economics that progresses in the manner of other sciences." The forward thrust is carried by the new concept which is nudged into being by stubborn facts inconsistent with the received theory. The case history is that of the commodity markets. Mr. Working points to six new concepts and describes the occasions of their birth. In the main, the account seems to me vivid and convincing. It exhibits the essential progression: the stubborn maverick fact; the new concept which tames it; the trail of more appropriate information, alignment, and enrichment of existing knowledge, tests (often with new techniques).

Obviously, the commodity markets do not afford an ideal example of scientific progress. The picture is mottled. Thus the concept of the contract, once formulated, could be embodied in statistics capable of giving it accurate form. The concept of hedging has not fared as well. To measure its month-to-month course, an awkward estimating procedure is required which may well lack the capacity to answer some important questions. Concepts 5 and 6 are perhaps in a still younger stage of scientific exploitation. Of these difficulties Mr. Working is well aware. They do not, I believe, call his conclusion into question. The matter at issue is not whether understanding of the futures markets provides an ideal illustration but merely whether it provides an illustration of the progress of scientific understanding. To substantiate the latter point one needs to be convinced that there is a clear correspondence between the requirements of the scientific process and what has actually taken place, and that it is feasible for the correspondence to be improved. Because the paper carries conviction on these critical matters, it seems to me to succeed in making its basic point and to give it the wallop that concrete illustration can supply. Accordingly, I want to enjoy the impact of Mr. Working's ideas and mention a few thoughts to which they give rise.

"The fabric of science," Conant says, is "the result of interweaving of fruitful concepts." If certain concepts are pillars of understanding in the futures markets, they must advance understanding wherever they touch the whole cloth of economic life. Mr. Working's concepts two and three indicate that futures and other markets touch quite intimately.

These concepts embody a central notion about commodity markets: the importance of businessmen (producers, merchants, processors, manufacturers) in contradistinction to traders in futures (speculators). Concept 2 says that "futures markets depend for their existence primarily on hedging." And hedging, virtually by definition, is what the businessman does when he deals in futures. Speculation merely "enters and leaves a well-established futures market . . . in extraordinarily sensitive and prompt response to the changing amounts of hedging." Who then is the hedger and why does he hedge?

Hedging is "a temporary substitute for a merchandising contract, without

specifying the purpose." Three categories of motives are distinguished: (1) to profit from divergence in the movements of spot and futures prices; (2) to simplify decisions; (3) to avoid loss or to profit from anticipated change in prices—selective or anticipatory hedging. Not too much seems to be known about each of the motives: by whom they are held, when, and the behavior they elicit. They are discussed by Mr. Working elsewhere. Businessmen activated by motive 1 appear to be a mainstay of some commodity markets. Their operations, typically short hedging, are often associated with heavy seasonal stock-carrying operations. Concept 2 is perhaps the classic fugitive from risk; perhaps he can be converted to 3 when he feels more sure of his guess about prices. A "long" position by the anticipatory trader is likely to be associated with an advance order for his manufactured product.

But even the simple listing of the several motives suffices to make an interesting point; except perhaps for motive 1, recourse to futures markets is only one of the ways in which the businessman meets a basic problem. The problem is how to lessen the cost of materials by selecting the time when buying prices are fixed via a futures contract or via the purchase of actual goods or in some other way. Advantageous timing of buying is a method of reducing the cost of materials just as is advantageous selection of qualities or delivery terms.

For the adequate understanding of either futures markets or the merchandise markets in which commodities are brought and sold, both markets must be understood in terms which can be readily transposed from one setting to the other. This intermarket relevance is clearly one of the ways in which new concepts, developed for futures markets, must fulfill their scientific function of, in Conant's words, "stimulating further experimentation or observation which in turn is fruitful." There are lessons to be learned on this score from Mr. Working's account. Consider a few of them.

The first lesson concerns, again, concepts 2 and 3: hedging. The central point is the importance of the futures markets to the businessman and vice versa. The concept should facilitate exploration of these relations. For this purpose it seems to me to have a flaw. Hedging, Working says, provides a temporary substitute for a merchandising contract. It is contrasted to speculation in commodities which is "the holding of a net long or net short position, for gain, and not as a normal incident to operating a producing, merchandising, or processing business."

Actually, people may hedge when they believe prices will rise (motive 3b) or fall (3a). For example, a cotton textile manufacturer has received a large advance order for grey goods to be delivered in three months. He expects that cotton prices will rise; therefore he buys cotton futures now. In making this decision he has rejected two alternatives: to buy cotton now or to do nothing. The fact that he does nothing means that he is betting on a rise in price. The fact that he does not buy cotton itself means he considers it wiser to make the bet in terms of a futures contract than in terms of actual cotton. He is, thus, half-identified and half-excluded by Working's definition of a speculator (given elsewhere): "speculation is the holding of a net long or net short position, for gain, and not as a normal incident to

operating a producing, merchandising, or processing business." The textile manufacturer's activity conforms to the first half of the definition because he does hold the position for gain; it is excluded by the second half because it is not "not as a normal incident to operating a . . . business." The issue is, what is speculation and what is normal? I propose that speculation, in the sense that I have described, is normal to materials purchasing in many businesses. In doing so, I admit that it is abnormal of me to use this dirty word in referring to good American business, but it would be still more abnormal for a businessman to be a chump and refuse to take a long position in materials when he is quite willing (and indeed praiseworthy) to take one in research talent or factory space. In terms of the definitions, then, "hedging" can be "speculating."

More than definitions are involved. The name that is used defines the problem under inspection and it tends in this case to throw out important parts of the problem; thereby hampering understanding. There is also a specific application in statistics of futures markets. The market distinction between hedging and speculation seems to refer to sorts of businessmen, not, as the names suggest, to what they do. Since improved reporting of participation in futures markets is greatly needed, it would be well to clarify whether it is the actor or the act that is to be designated. If the actor, the names should somehow refer to the businessman versus the trader. If the act, a designation is needed that is neither misleading nor tainted by the businessman's fear of being called a speculator when he is actually simply making the best of the fact that one way or another he needs to take account of the probable course of materials prices. This would clarify, I think, rather than alter Working's concepts 2 and 3. It would certainly provide a smoother bridge between futures and merchandise markets, and facilitate for both the straightforward examination of the role of price anticipations in economic change.

Consider now the intermarket relevance of concept 1, the open contract. An open contract initiates a long and matching short position which lasts until the contract is closed by the delivery of goods or offset in some other way. It is basically a balance-sheet concept in contrast to the replaced notion of buying and selling. In this sense it is analogous to a deposit in contrast to bank debits.

Transposed to merchandise markets, the corresponding concept is outstanding orders (the long position) and unfilled orders (the matching short position). The analogue of the replaced concept is perhaps new orders other than those for immediate delivery.

Now of course new orders for other than immediate delivery cannot be a replaced concept because it has never held sway. Even total new orders fights for its position relative to shipments or production, and we still say that new orders "lead" production rather than what somehow seems more obvious that production "lags" new orders. Actually, I think that far from needing to replace the flow concept of new orders in merchandise markets, work in the commodity markets highlights its importance relative to other flow (not stock) concepts. The analogy to bank debits shows why. New

York City bank debits are a confusing mixture of exceedingly high velocity financial payments and the more deliberate payments for goods or services; the same confusion is present in the fast turnover of "scalper" traders in contrast to the slower pace of hedging transactions. But the problem is not nearly as serious for debits and deposits outside of New York City; nor for merchandise (rather than futures) markets. There are other reasons too why flow concepts are necessary in markets for real goods.

Here, then, the lesson to be learned from the progress of scientific inquiry in the futures markets is what questions to ask about expectation-based changes in holdings. To answer it, concepts need to be changed, but they require revision rather than replacement; and revision is another recognized scientific implement.

In merchandise markets the true "defender" against the "challenger" concept of the contract (renamed here the outstanding order) is stocks of purchased materials. Obviously a long or short position can be realized in terms of stocks on hand as well as on order. To include both sorts, we need the concept of purchased materials stocks on hand and on order; that is, total ownership of materials. Without pointing out the extent to which this is and is not an analogy to the contract, my thought is simply that its use would facilitate a transfer of knowledge between manifestations of materials purchasing problems in futures and merchandising markets.

Concepts 4, 5, and 6 deal with continuity in, and rationality of, markets over time. They concern the power of expectations, particularly the expectation of the professional trader, to produce this integration in what I dare say is primarily seasonal variations in supply, demand, and costs. The professional trader is visualized as able to judge when the hedger, driven perhaps by the business problems that lead him to hedge or by fallacious judgment, overshoots the mark. Accordingly, he chooses to go "long" when the hedger wants to go "short." A choice between spot and futures is also provided. Furthermore, the experienced trader moves freely from one futures market to another. The net result of at least this educated aspect of the open markets should be some reduction in fluctuation, other things the same, for spot as well as future prices, a proper relation between today's and tomorrow's prices, and perhaps among prices for different commodities. The uneducated aspect, or the aspect that responds to similar business necessities or expectations, may have the reverse effects—that of augmenting fluctuation. Obviously, in all this, futures markets have an important bearing on merchandise markets and vice versa, and there are fascinating methodological as well as factual aspects to be explored.

Let me simply say in conclusion that Holbrook Working's pursuit of scientific goals in his years of work in commodity markets should be a heartening example for all who aim, like Working, not merely at tests but at creative questions.

MICHAEL J. BRENNAN: In a note attached to his manuscript, Professor Working told me that another discussant "expresses enthusiastic concurrence with my methodological views. If contrary opinion is to find expression in the

discussion, it will apparently have to come from you . . . to enliven the proceedings, so far as concerns attacks on my somewhat heterodox ideas." In one respect I am afraid I shall have to disappoint him. There is no doubt in my mind that the new concepts, introduced by Working and others, have vastly improved our understanding of futures markets. The concepts are obtained from objective observation. That is, they are nonintrospective, or what I would call nonintuitive. One examines statistical tables or interviews participants in the market, forms a concept to describe what traders are doing, and then applies the concept to other data in order to test whether it describes their behavior. Though I may have a quarrel with minor aspects of the concepts—such as the isolation of speculators from dealers, implying one person cannot be both—in general, I believe he has demonstrated, both here and elsewhere, the usefulness and the validity of his concepts.

However, there is another respect in which I heartily accept his invitation to do battle. Indeed, I feel compelled to do so. If my sleep is to be undisturbed tonight, I must do my best to disturb his. My disagreement is centered on his generalization to all of empirical economics and its methods of inquiry. Perhaps my criticism should be directed toward James Conant. But I shall take Professor Working to task for disseminating what I regard as a myopic, distorted notion of scientific method, and in particular as it applies to economics.

First, few economists would deny that economics has both a positive (scientific) aspect and a normative (ethical or philosophical) aspect. Yet this in no way prevents positive economics from being as scientific as the natural sciences. In place of controlled experiments we have statistical analysis; the same is true of certain of the natural sciences and of certain facets of others. And if we were to carry the comparison to its conclusion, natural science, too, has its normative aspects—or, if one prefers, its aspects of a practical art and a social philosophy. Witness the social and moral problems of the application of nuclear energy, the uses to which poison gasses can be put, etc. Certainly, the distinction between an impassionate discovery and the policy uses to which it can be put is irrelevant to the classification of a discipline as scientific.

Second, even the positive aspect of economics is denied the appellation "science" because of "the long-standing addiction of economic theorists to what we now call model building." Contrasted to the model is the concept, obtained from empirical observation. Now a model is nothing other than a theory, a system of explanatory propositions that relates each concept in the system to the others. What Professor Working has offered us is not just six concepts but a model or a group of models. To illustrate, his multipurpose concept of hedging necessarily implies that dealers are maximizing expected profits. Hedging by dealers does not stand alone as its own rationale, sufficient unto itself. It and other activities of the dealers are related to profit maximization—another concept. Because the application of the new hedging concept to statistical data has not explicitly spelled out its origin in profit maximization does not destroy the relation between the two. It means merely that the complete model has not been written out in explicit, systematic steps. Since the objective of a dealer, not yielded by the data, is there in im-

plicit form, its inclusion as an assumption of a model can hardly be called unscientific.

Perhaps what Professor Working has in mind is the penchant of some economists to engage in mathematical exercises that have little bearing on significant economic problems. If so, I am deeply sympathetic. But I would point out that trivial and empirically invalid models can be found in the natural sciences as well as in economics. It is a little unfair to compare Boyle with a mental gymnast! Even in this connection, however, one must be careful to distinguish among varieties. Some models are built for the very legitimate purpose of discussing their logical properties. From abstract axioms the existence and conditions of equilibrium may be inferred, for example. In empirical work (by others?) this can provide a safeguard against subtle errors of reasoning or the drawing of unwarranted conclusions—errors that have been committed mostly by those who make direct application of a concept to empirical data. Another type of model building is that in which axioms, not objectively observed, form the foundation for predictions that are objectively observable, which brings us to my third criticism.

It has been argued that economists, whenever they have developed a new concept based on observation, have tended to present the concept as a deduction from some basic principle. Rather, it is suggested, the concept should be treated as a consequence of the observation itself. Why? Because this is what natural scientists do. We cannot proceed to examine this contention without introducing some clarification. There is some ambiguity in the meanings of concept, basic principle, and observation. In one sense, all human concepts are based on, or begin with, observation. However, observation as used here must be interpreted to mean objective observation, as these terms were described earlier. Basic principle I take to mean a basic, nonobserved or nonobservable, assumption. "Concept" I shall interpret, not just as a variable, but as an explanatory theoretical proposition subject to empirical test. I believe these meanings are consistent with the spirit of the paper.

Now let us rephrase the argument by reference to examples drawn from economics and the natural sciences. The demand schedule is a concept—a theoretical proposition based on observation and subject to empirical test. When newly discovered, economists presented the demand schedule as a deduction from utility maximization, a nonobservable basic assumption. They should have presented it as a consequence of objective observations on their own behavior, their neighbors' behavior, and negative correlations between quantity consumed and price found in statistical tables.

Next, consider the other side of the coin. When physicists presented the concept of the interaction of atoms, they stated that the concept was simply the consequence of microscopic observations on the behavior of atoms. They did not attempt to deduce atomic behavior from subatomic general principles, such as the interaction of nonobservable electrons, protons, etc. Or again, quantum mechanics made no reference to equations as axioms of the theory, equations that in fact are now being used and cannot even be interpreted in terms of objective observation.

The point intended by the illustrations should be clear. Professor Working's

economics of empirically based concepts would leave us with a demand schedule that is a mere summary of observations, not a theory. In the cases of the natural sciences, the above statements are obviously false. I think Working has been led astray by the ill-founded generalizations of Conant, and this is perhaps itself an example of what can happen from enchantment by empiricism. Scientific research is theoretical *and* empirical, where empirical research includes quantitative measurement and/or tests of theory. Scientific theory is explanation of phenomena consisting of general assumptions and specific predictions. The predictions must be testable by Working's objective observation; i.e., they must be empirically based concepts. But the assumptions need not be. In fact, they often are not, neither in economics (utility maximization) nor in natural science (entrophy maximization). Their purpose is to render generality by abstraction from the contingencies and complexities of the data. The more verified predictions obtainable from an assumption, the better is the assumption. And historically speaking the best assumptions are often the most abstract. Were one to take Conant's prescriptions literally, human knowledge would be set back at least a thousand years!

In conclusion, let us evaluate the position of Working's six new concepts. Tribute has already been paid to their fruitfulness. Regarded as hypotheses, the evidence indicates that they should be accepted until better ones are found. Of course, this is true of all verified hypotheses. Moreover, these hypotheses are derived directly from observation. Their success, however, provides no justification for barring hypotheses derived from intuition. Any source is legitimate. From a scientific viewpoint, the only requirement is that predictions deducible from the hypothesis be subject to test, that they be empirically based, granted that no logical errors have been committed in deducing the predictions from the assumptions. To exclude intuitive hypotheses on any other grounds renders science less, not more, dynamic.

MARC NERLOVE: In my discussion of the papers by Professors Working and Cootner, I should like to place some of what they have to say on futures markets in the broader context of problems common to applications of theories of behavior under uncertainty. It is useful, in this regard, to contrast theories of choice under uncertainty with theories of choice under certainty.

Theories of choice under certainty, or conditions of perfect foresight, form the basis of a large part of economic analysis. Such theories have three essential elements: (1) a preference ordering, for each individual, of the consequences of his possible actions; (2) an opportunity set which restricts the choice of actions open to him; (3) a specification of the way in which individual choices combine to produce a social outcome. In most of our theory the last element will generally take the form of a theory of markets.

The chief characteristic of uncertainty, on the other hand, is that the consequences of action are not completely known at the time a choice must be made. One effect of the introduction of uncertainty is to blur the distinction between tastes and preferences on the one hand and opportunities on the other. Another effect is to make information a valuable commodity requiring the input of scarce resources in its production, and to justify the rule, "Never do

today what you can put off until tomorrow"—when information will presumably be cheaper.

In a theory of individual behavior under uncertainty, the problem is to define a rational course of action when the outcome of the action is unknown. The importance of the study of futures markets lies in the fact that it opens the broader vista of social behavior under uncertainty. But to stay with the problem of the individual for a moment: The various different theories we have today are based essentially on assumptions which relate choices under uncertainty to choices under certainty. For example, the notion of certainty equivalence is such a theoretical approach. It has played an important role in the study of investment and supply decisions—decisions which are largely individual in character. The idea of a certainty equivalent is this: For any uncertain variable we find another variable, the value of which, if expected with certainty, would lead to the behavior which occurs under conditions of uncertainty. Risk premia, which play an important role in Cootner's discussion, are an especially simple example of certainty equivalents. The general approach of certainty equivalence has shortcomings even when applied to individual behavior; I think these are of even a more serious and more fundamental character applied to social behavior under uncertainty.

In the study of futures markets, markets for securities and for money, and the like, we are concerned with the institutional structures which develop to handle the social problems arising from uncertainty. Markets are, of course, one of a number of possible institutional solutions to these problems. When we see a market of any kind, the first step in our analysis should be to find out what is being exchanged. As Working has so forcibly stated, ownership of commodities is not what is basically exchanged in futures markets. Since exchange rests ultimately on individual differences and on division of labor, looking for these may aid in the determination of what is exchanged. The effects of both individual differences and division of labor can be found in the development and operation of futures markets.

In "The Scope and Limits of Futures Trading," published last year, Houthakker distinguishes between social uncertainty (that due to the fact that many individuals take part in production and consumption) and individual uncertainty (that present when not all participants know exactly what they will do in the future under various unknown states of the world). Purely social uncertainty, in Houthakker's sense, can be eliminated by forward trading; that is, by consummating all transactions, present and future, for all time now. Individual uncertainty, on the other hand, cannot be eliminated in this way but only transferred, in whole or in part, from one group of individuals to another. Because markets which can perform these functions are not themselves costless, the extent to which these uncertainties are transferred or eliminated is limited by transactions costs. Working has brought this aspect of futures markets out especially clearly in many of his other papers on the subject.

The first two basic concepts which Working has brought out in the present paper reflect the idea that futures markets are not gambling arenas but perform a useful function of partially transferring and partially eliminating the various kinds of uncertainty. The element of transactions cost, which has not

been discussed in the present paper, helps to explain why the transfer or elimi-
nation of uncertainties is only partial; why, for example, trading is done not
in forward contracts which are very precise in their delivery terms, but in
futures contracts which are a great deal vaguer. The reason, to paraphrase
Adam Smith, is that transactions costs are "limited" by the extent of the
market, the greater the extent the lower the costs. In the same way, one can
explain why futures trading tends to be confined to a very few types of con-
tracts in one or two major market centers; why, for example, Seattle merchants
hedge their stocks in Chicago futures for an entirely different kind of wheat.
Transactions costs also explain why a short hedge is only imperfect protection
against the risks of holding stocks and why stocks are frequently held only
partially hedged.

Working's third basic concept is that hedging is done for a variety of pur-
poses and not solely to avoid risk. I must confess that I found this concept
difficult to understand—perhaps because of my prejudices—and hard to re-
concile with what Working says in this paper and elsewhere. If not to reduce
social uncertainty and transfer individual uncertainty, what then? Working's
answer is that hedging is done: (1) with a view towards profiting from di-
vergent movements in spot and futures prices; (2) to simplify business de-
cisions and cut costs; (3) to profit from expected price changes. It seems to
me that all these reasons reflect not the absence of a desire to transfer or
eliminate certain kinds of risk but only in part imperfections in the ability to
do so, costs of doing so, and in part the effects of a kind of informational di-
vision of labor. Short hedgers, for example, transfer to long speculators, not
only some of the price risks attendant on holding stocks, but also a large part
of the job of anticipating over-all movements in prices. They reserve for them-
selves the task of anticipating movements in the basis; that is, the difference
between the futures and the spot prices. All this, of course, raises problems
with the definitions of speculation and of hedging. Working's attempt does not
seem especially successful because the distinction he makes does not rest on
a distinction of functions.

Working's next two concepts are a good deal less basic than the first three,
if we view them against the backdrop of market solutions to social problems
of uncertainty. They are nonetheless important for the study of the storage of
nonperishable commodities and the functioning of markets for futures in these
commodities. As pointed out by Cootner, these market solutions are intimately
bound up with the problems of intertemporal price equilibrium.

In concept 4, Working holds that, within any period over which stocks are
carried, the differences between spot and futures prices reflect the costs of
storage net of the convenience yield of stocks. Although convenience yield
arises primarily because of uncertainty, uncertainty does not basically enter
this concept of Working. In Cootner's formulation, uncertainty plays a more
basic role because Cootner qualifies Working's position by adding a risk
premium.

In concept 3, Working holds that each futures price is an unbiased estimate
of the price which could reasonably be expected to prevail during the appropri-
ate future month on the basis of contemporarily available information. As

such, Working's concept of what the future and price represents is quite similar to Muth's "rational expectations" which are discussed in a forthcoming paper. Cootner would not accept this position, however, since the existence of a risk premium implies that futures prices are downward biased expectations.

Just what Cootner does hold about expectations is difficult to determine; at one point he assumes "that all parties expected the prices that actually came to pass." If this were consistently the case, it seems to me that futures markets would, to paraphrase another eminent economist, soon "wither away." In any case, if one must simplify by introducing certainty equivalents, differences in the values of these are surely in the essence of futures trading, a fact which Cootner later makes use of.

Working's final proposition concerns the absence of "normal backwardation," which is what Keynes and Hicks termed the situation in which the futures price lies below the spot plus storage charges. Its presence or absence bears on the existence of risk premia.

Before drawing any general conclusions about the papers before us, I would like to raise a specific issue concerning what Cootner regards as the best evidence for his version of the price of storage hypothesis. Following earlier studies by Telser and Brennan, Cootner estimates the supply of storage function by a regression of the basis on stocks. He is thus minimizing deviations of the futures minus the spot price from this line. For this to be an appropriate procedure, it must be true, not only that the supply of stocks is highly inelastic, but also that the supply of hedged stocks is too. While the first is an entirely plausible assumption, the second is not in view of the costs of hedging which both Cootner and Working have brought out. It follows that the difference between future and spot is determined jointly by the supply of futures (equal to net short hedging) and the demand for futures (equal to net long speculation). Even if speculators' expectations were very nearly the same, the demand for futures contracts would not be nearly perfectly elastic because of the differing asset positions of speculators. Hence, a regression of stocks or net short hedging on the basis would not be appropriate either. Furthermore, it is not appropriate for the same reason to interpret residuals from the regression of the basis on stocks as purely the result of changes in speculators' expectations. In short, statistical procedures should reflect the underlying interdependence. Cootner's do not. Despite this objection, however, his evidence remains impressive.

The main general conclusion I would draw from these two papers is that they appear to represent a tendency in the study of futures markets to minimize the role of uncertainty. In order to simplify, we are perhaps tossing the baby out with the bath. Allowing uncertainty itself to play a more fundamental role is not likely to be easy. Even the notion of certainty equivalence which has proven so useful in other areas seems misapplied in the study of those markets which have developed in order to reduce or transfer uncertainty. Perhaps the up-in-the-air feeling these papers leave me with, however, is just the result of their being truly on the frontier of economic knowledge.

DISTRIBUTION COSTS: CONCEPTS AND MEASURES

HOW MUCH DOES IT PAY WHOM TO ADVERTISE?

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Exploring reasons why there are differences in the extent to which consumer products are advertised is the purpose of this paper. Kaldor and Silverman found that advertising as a percentage of sales is remarkably similar by product in the U.S. and U.K.[1] This suggests that there are characteristics of products and markets that can explain different advertising intensities among products. Of course, what is nominally called advertising forms only a small part of the total resources intended to make buyers aware of goods and services. That advertising and personal selling services are alternative methods of persuading buyers is apparent when we recall the growth of self-service stores in which consumers choose among packaged, branded, and advertised goods without persuasion by retailers. No doubt commodities differ much less with respect to the proportion of their price representing the total cost of informing and persuading consumers than with respect to relative advertising expenditures for the same reasons; for example, that not all products are advertised on television. Thus a more inclusive measure of selling costs would reveal less differences among commodities than a less inclusive measure such as advertising outlays. Nonetheless, the explanation of differences in the relative advertising expenditures among commodities poses a challenging problem to which this paper is directed.[2]

The point of departure is an important proposition due to Dorfman and Steiner: a firm maximizing profit spends an amount on advertising and chooses a price such that the price elasticity of the demand for its product equals the value of the marginal sales effect of advertising.[3] To prove this and to obtain other propositions about advertising we need to perform an exercise in the economic calculus.

Let

(1) $$C = G(q)$$

[1] N. Kaldor and R. Silverman, *A Statistical Analysis of Advertising Expenditures and of the Revenue of the Press* (Cambridge Univ. Press, 1948), Table 10.
[2] For a discussion of this topic see N. H. Borden, *The Economic Effect of Advertising* (Irwin, 1942), pp. 422-38.
[3] R. Dorfman and P. O. Steiner, "Optimal Advertising and Optimal Quality," *A.E.R.*, 1954, pp. 826-36.

represent total production cost as a function of the rate of output, q.
Let

(2)
$$N = I(x)$$

give the relation between N, the number of consumers made aware of the product, and, x, the amount the firm spends on advertising. One simple measure of consumer awareness of a brand might be the number who can name the brand on unaided recall. Thus the cost of making N consumers aware of the product is x and the marginal cost of awareness (m.c.a. for short) is

$$1 \div \frac{\partial I}{\partial x}.$$

The sales of the firm vary directly with N, the number of consumers made aware of its product and inversely with its price p. Thus the demand function is

(3)
$$q = F(p, N).$$

We see that the amount the firm spends on advertising enters the demand function via equation (2).

The firm chooses x and p to maximize its net revenue $R = pq - C - x$. The necessary conditions for maximum R are that

(4)
$$\frac{\partial R}{\partial x} = \left(p - \frac{\partial G}{\partial q} \right) \frac{\partial F}{\partial N} \frac{\partial I}{\partial x} - 1 = 0$$

(5)
$$\frac{\partial R}{\partial p} = \left(p - \frac{\partial G}{\partial q} \right) \frac{\partial F}{\partial p} + q = 0$$

The elasticity of the demand for the product is

(6)
$$-\eta = -\frac{p}{q} \frac{\partial F}{\partial p} = \frac{p}{p - \dfrac{\partial G}{\partial q}}$$

The value of the marginal sales effect of advertising is

(7)
$$p \frac{\partial F}{\partial N} \frac{\partial I}{\partial X} = \frac{p}{p - \dfrac{\partial G}{\partial q}}$$

Hence we see that the price elasticity of demand equals the value of the marginal sales effect of advertising, the Dorfman-Steiner theorem.

Equation (4) has an interesting interpretation. The marginal sales effect of awareness (m.s.e.a. for short) is

$$p\,\frac{\partial F}{\partial N}\,.$$

Since not all consumers aware of the firm's product purchase the same amount and, indeed, some consumers aware of the product do not purchase it at all,

$$\frac{\partial F}{\partial N}$$

generally is not constant.

Define one minus the ratio of the marginal production cost to the price to be marginal advertising intensity, m.x.i. for short. Thus the marginal advertising intensity is

$$(8) \qquad\qquad \text{m.x.i.} = \frac{p - \dfrac{\partial G}{\partial q}}{p}$$

The rule for finding the optimal advertising expenditure is that the marginal advertising intensity times the marginal sales effect of awareness equals the marginal cost of making consumers aware of the product; i.e.,

$$(9) \qquad\qquad \text{(m.x.i.) (m.s.e.a.)} = \text{m.c.a.}$$

Assuming a fixed price, the number of consumers a firm will wish to make aware of its product is found by solving (9) for N. In geometric terms the marginal cost of awareness is represented by a curve that probably increases as N increases. The marginal return from awareness, (m.x.i.) times (m.s.e.a.), is represented by a curve that is likely to decrease as N increases. For example, the number of consumers aware of a firm's product may be so large that the additional revenue obtainable by increasing the number of aware consumers is very low. Thus there is likely to be diminishing returns and increasing cost of awareness. Equation (9) shows that the optimal N and therefore optimal x is determined by the intersection of the two curves: the marginal return from awareness and the marginal cost of bringing about awareness.

Casting the argument in terms of the number of consumers made aware of a product or brand name makes it possible to compare advertising of products with respect to a natural unit, N. The amount spent on advertising will depend on the level and shape of the two schedules: the marginal cost of making consumers aware of a brand and the marginal return from awareness. For instance, a product much

discussed by consumers is one for which the firm need spend little to obtain a given level of awareness.[4] For such a product the marginal cost of awareness is likely to be low and the slope of m.c.a. with respect to N is likely to be small.

How long will consumers remain aware of a brand? Consumers tend to forget brands and continuous advertising is needed to maintain a given rate of sales. Thus advertising expenditures can be viewed as a capital good that depreciates over time and needs maintenance and repair. New consumers continually enter the market and firms need to advertise continuously to make them aware of their products. It would be easy to take these dynamic factors explicitly into account in the formal model but this task is not pursued here. The effects of these factors and other aspects of commodities and markets explaining differences of advertising expenditures among products is illustrated later. Now we shall see how price elasticities have an important role in explaining differences of advertising intensities among products.

The smaller the difference between the marginal production cost and the price, the smaller is the marginal advertising intensity. Moreover, from (6) it follows that the marginal advertising intensity and the price elasticity vary inversely. Now suppose that marginal production cost is nearly constant and, therefore, nearly equals average variable cost. Hence the marginal advertising intensity is approximately one minus the ratio of the average variable cost to the price. Total revenue equals the sum of total variable cost, the advertising outlay, and the maximum net revenue. Hence the ratio of the advertising outlay to sales, the advertising intensity, does not exceed the marginal advertising intensity. In symbols

$$(10) \quad \text{m.x.i.} \approx \frac{(p - \text{average variable cost})}{pq} q = \frac{R + x}{pq} \geq \frac{x}{pq}, \qquad R \geq 0$$

However, the marginal advertising intensity equals the reciprocal of the price elasticity. Therefore the price elasticity does not exceed the reciprocal of the advertising intensity. Algebraically,

$$(11) \qquad\qquad \eta = \frac{1}{\text{m.x.i.}} \leq 1 \div \frac{x}{pq}$$

This means that if average variable cost is nearly independent of scale then the reciprocal of the advertising intensity is an upper bound to the price elasticity. Thus, for example, if advertising outlay is one-half of total sales, an advertising intensity of one-half, the price elasticity at the optimal output is between one and two. Or, if the advertising intensity is one per cent then the price elasticity is less than 100. Al-

[4] S. A. Ozga, "Imperfect Markets through Lack of Knowledge," *Q.J.E.*, 1960, pp. 29-52.

though the advertising intensity of two products may differ at the optimum price and advertising outlay, the price elasticities might be the same; the theory merely asserts that the reciprocal of the advertising intensity is an upper bound to the price elasticity. However, differences between the upper bounds of the price elasticities suggest that there are similar differences among the elasticities. This is clearly an empirical and not a theoretical proposition to be determined by the facts. If by advertising a firm reduces the elasticity of the demand for its product, then price elasticity and advertising intensity would vary inversely. An empirical finding that there is an inverse relation between advertising intensity and price elasticity need not, however, rest on such an immediate relation between the two. Because the reciprocal of the advertising intensity is an upper bound to the price elasticity given the assumed cost conditions, it is asserted that price elasticities of heavily advertised products are lower than of little advertised products. An implication of this proposition is that in a perfectly competitive market because the firms cannot affect the price they will not advertise. That advertising serves no purpose in such a market follows from the tacit assumption that there can only be a single price since buyers and sellers are prefectly informed about all offers.

A plausible case for the inverse relation between price elasticity and advertising intensity can be made for cost conditions other than constant a.v.c. Suppose marginal production cost exceeds average variable cost, implying the latter increases with scale. Hence

$$(12) \quad \frac{1}{\eta} = \text{m.x.i.} = \frac{p - \dfrac{\partial G}{\partial q}}{p} < \frac{p - \text{average variable cost}}{p} = \frac{R + x}{pq}$$

and

$$(13) \quad \frac{p - \text{a.v.c.}}{p} > \frac{x}{pq}$$

Thus the average advertising intensity is probably closer to the marginal advertising intensity assuming increasing average variable cost than assuming constant average variable cost. Hence the easily measurable number—the ratio of sales to advertising outlay—may be even closer to the price elasticity (though it is no longer an upper bound to the elasticity) for increasing than for constant marginal production cost.

This analysis leads us to predict that heavily advertised products should exhibit lower price elasticities than little advertised products. Although it is relatively easy to measure the ratio of advertising to sales, there are few estimates of the price elasticity of firm demand schedules and the current state of knowledge does not allow a precise

test of this prediction. However, considering what products are heavily advertised lends it credence. Judging from the *Statistics of Income* the most heavily advertised products are perfumes, cosmetics, other toilet preparations, drugs, and patent medicines. It seems plausible that the firms making these products face demand schedules of rather low elasticity. These products serve delicate needs and consumers are often firmly attached to particular brands to avoid risking health or beauty on untried alternatives. Consumers who are convinced that a particular toothpaste prevents tooth decay or bad breath are unlikely to try another that costs less but lacks this property. That rare lipstick of the right shade that might lead to or save a marriage is almost priceless. The consumer of a shaving cream that assuages a sensitive skin will not incur pain and risk complexion by switching to a cheaper brand possibly deficient in these respects.

Obviously consumers do not disregard price when choosing among rival brands. Lower priced and less-advertised brands manage to survive in these product classes although they may not seize large market shares. The continued availability of cheaper brands in some product classes suggests that not all consumers are convinced of the merits of the heavily advertised brands and that consumers can freely choose to pay for advertising if they buy such heavily advertised brands.

Products sold under highly competitive conditions are little advertised. Thus many food products including some canned goods, fresh fruits, and fresh vegetables are not intensively advertised by producers. That some of these products are advertised by co-operatives serves to reinforce the prediction relating advertising intensity and price elasticity. The price elasticity relevant to a marketing organization of this type is that for the product as a whole and not the elasticity of the demand schedule for a particular producer. Since the latter is much higher than the former, it might pay for the co-operative but not for the individual firm to advertise.

Although the argument linking price elasticity to advertising intensity helps explain why some products are heavily advertised, there are others for which it does not seem to apply. These are heavily advertised products sold by firms facing highly elastic demand schedules. A case in point is cigarettes. For most of the period 1916 to 1952 retail prices of leading cigarette brands were the same. Although because of this it is difficult to estimate the cross-elasticities among the brands, the absence of price differentials in itself suggests high cross-elasticities. If, on the contrary, the cross-elasticities were low, then the brands could bear different prices and survive. There is, moreover, some pertinent evidence in the history of the cigarette industry that supports the view that the cross-elasticities were high. In one instance it is reported that

Lucky Strike increased its retail price relative to the other leading brands; during the six-week period of this price differential and even shortly afterward, Lucky Strike sales fell sharply. Nicholl's calculates an elasticity of about —2.9 for this episode which understates the elasticity because it is based on wholesale instead of retail prices and the percentage change of the latter was smaller.[8] During the early thirties, the ten cent brands made large inroads on the sales of the three leading brands (Camels, Lucky Strike, and Chesterfield) until the latter reduced their price, albeit not to the level of the cheaper brands. This evidence and the absence of price differentials among the leading brands implies fairly high cross-elasticities among cigarette brands. If this is true, then the fact that cigarettes are heavily advertised might contradict the argument that price elasticity and advertising intensity are inversely related. There are, however, other facts pertinent to the explanation of the advertising intensity of cigarettes. First, from evidence relating total cigarette sales to average price it appears that the over-all price elasticity of cigarettes is not high. Second, total cigarette sales increased substantially during this period chiefly because of the increase in the number of female smokers but also because of some increase in the number of male smokers. In theoretical terms, the number of consumers made aware of cigarettes was increased.

Given high cross-elasticities among cigarette brands, the effect of a price reduction by one brand is to increase its sales primarily at the expense of the sales of its rivals. Because the over-all price elasticity is low, a price reduction by one brand will not increase total sales very much. Price cutting could continue nonetheless until a level is reached at which the price equals average cost. At that price no firm can obtain monopoly profits. Agreement to maintain a higher price gives the firms at least the possibility of obtaining monopoly profits. However, even if there were only a single firm in the industry, e.g., the American Tobacco Company before 1912, because advertising makes possible an expansion of the market by convincing some nonusers to try the product, advertising might be profitable. Thus there are circumstances in which a monopolist would find profitable advertising that attracts new clientele. Given several firms in the industry, there is all the more reason to expect the amount these firms spend on advertising to be at least as large as the monopoly amount, since there will be more brands to inform buyers about. However, the competitive effects of advertising differ from the effects of a price reduction because the firms are thereby able to compete for new business. If one firm cannot advertise as effectively as another, it suffers to the extent that it fails to obtain as large

[8] W. H. Nicholls, *Price Policies in the Cigarette Industry* (Vanderbilt Univ. Press, 1951), pp. 50-51.

a share of the new business as its more efficiently advertising competitors; its absolute sales and profits are not necessarily reduced. On this view, the price policy that is analogous to the effects of increased advertising is not a price reduction given to all customers but is instead one made available to only the new customers. Due to the cost of ensuring that only the new customers obtain the price reduction, advertising can be a more effective way of expanding the market.

There are nonetheless competitive effects of advertising analogous to price competition. The best of all solutions for the industry would be a marketing organization that sells the product for the group, shares the profits, and advertises for all. We can invoke the antitrust laws to explain why most combinations resort to greater subtleties and come close to the same effect.

This argument clearly applies to the strategy of a cartel. Suppose there are a few firms in an industry who collude to obtain monopoly profits. The preceding analysis shows why such a combination would not suppress advertising, since that would have the effect of limiting the expansion of the market. Explicit collusion on price is not essential. If price changes by one firm are promptly met by the others, then the relevant firm demand schedule is the *mutatis mutandis* demand. Thus at any price the elasticity for the group and the firm are the same. Although the cross-elasticities among brands may be high, the advertising intensity varies inversely with the price elasticity of the group demand schedule. Oligopolies are suspect and price competition, chaotic or otherwise, unlikely. The price and the advertising outlay in such an industry will not depart widely from the monopoly solution.

This argument leads to the conclusion that commodities of certain characteristics and produced under oligopolistic conditions are likely to be heavily advertised. First, there must be few firms in the industry and cross-elasticities must be high. The latter implies that if one firm reduces its price and the others do not follow, it can increase its sales by a large amount at their expense. Second, the price elasticity for the product must be low. Third, there must exist a large potential market that can be realized by means of advertising. Cigarettes are of course the classical example of a product with these characteristics. Do soft drinks, chewing gum, and razor blades also have these characteristics? The latter is particularly interesting because new customers can often obtain special price reductions not available to the regular customers when blades and razors are tied into a single offer unappealing to regular users.

This argument also explains why some products are little advertised because some of the required conditions are absent. Perhaps two good examples are salt and sugar. Over-all price elasticities for these prod-

ucts at their current price are probably low and price differentials among brands is small, suggesting that cross-elasticities among brands are high. Concentration in these industries is high. The products are widely used, and it is doubtful whether advertising could expand the market or persuade people to increase their consumption. Salt, however, differs from sugar in some less obvious respects because it can be iodized, thus preventing goitre, and made to pour freely even when it rains. As to the latter, consumers can judge for themselves, and all firms can easily iodize their salt. Unfortunately sugar firms have not yet discovered how to differentiate their product or to overcome the taste for a svelte figure. Even if there were collusion, tacit or otherwise, firms in these industries probably stand to gain little by advertising, since it would mainly affect their competitive positions without expanding the market. If there were a cartel in these industries, it might be well advised to suppress both price and advertising competition.

These arguments show why oligopolies producing certain kinds of products might advertise intensively. It does not imply the converse that advertising can explain the competitive structure of the industry. The converse proposition requires the demonstration that advertising is a barrier to entry or that, for a given market size, the use of advertising so increases the minimum viable firm size that the industry can contain but a limited number of firms.

There are at least two ways in which advertising might inhibit entry. First, it may affect consumers insidiously, so that their attachment to particular brands becomes so strong that new brands cannot gain a foothold. I would not be surprised if sanitary napkins are in this enviable position. Second, advertising, like machinery, is expensive and the capital market is said to be imperfect. In addition, there must be increasing returns to awareness or decreasing cost of creating awareness. Whatever truth may lie in this viewpoint, we know that even in some industries in which products are heavily advertised, nonadvertising firms manage to survive, perhaps because there are consumers who choose not to buy the more heavily advertised brands.

There are some heavily advertised products for which explanation of advertising intensity requires an examination of the dynamic character of the markets. These are commodities for which product changes are frequent, new brands are often introduced, and the life span of many brands is short. Consumers' knowledge of such goods tends to become rapidly obsolete, and firms find it advantageous to invest heavily in persuasion and information.

An important function of advertising is the provision of information by sellers to buyers about the nature of products and the terms of sales. The faster the turnover of buyers and the larger the number of new

potential buyers the more advantageous do sellers find advertising. Thus baby foods and other children's products, for example, are heavily advertised. Sellers need to inform buyers about changes in the terms of sale and this in itself explains the pervasiveness of advertising. Much newspaper advertising concerns "sales." If in addition sellers change the character of existing products or offer new products, then consumers' current information about products becomes obsolete. Thus new brands or old brands that have been modified are more intensively advertised than static brands.

Some products are of a kind that some consumers become restive with existing brands and are prepared to try new varieties. Breakfast cereals are a possible example. Although some brands have persisted, many have short life spans. Because, unlike beer, cigarettes, and many other consumer products, consumers can readily perceive differences among brands of breakfast cereals; they can grow accustomed to particular ones. Repetition dulls their palate and leads them to try new varieties. Were it not that consumers value variety, the few firms in the breakfast cereal industry would provide fewer, less-advertised, and cheaper brands.

Soaps resemble breakfast cereals in some of these respects, but the intensive advertising is also due to product changes. In recent years, for example, detergents and liquid soaps have been introduced. Soap firms advertise to make consumers aware of new brands and changes of old brands. A white bar of Ivory soap connotes purity: will a white plastic bottle of liquid Ivory connote the same purity? We cannot envy the problems facing the soap industry.

Many other products are as subject to fads and style changes as soaps and breakfast cereals. Clothing, books, and motion pictures are ready examples. The latter, as expected, are about as heavily advertised as soaps and breakfast cereals. Although new books appear continuously and fads are prevalent, books are not as intensively advertised as movies. To be sure, books receive much free publicity in newspaper and magazine reviews and, what may be more important, are a popular topic of conversation for the book-buying public. Thus potential buyers of books exchange much information among themselves at no cost though not necessarily to the advantage of booksellers and publishers. In these respects, books and movies are similar. They differ in the size of their potential markets. A successful movie is viewed by upwards of twenty million people, but the sales of few books exceed one hundred thousand. If there is increasing marginal costs of awareness and the marginal return from awareness is greater for movies than for books, then movies will be more heavily advertised than books.

Clothing styles change frequently but new styles are not introduced

as often as new books or movies. Hence we should not expect clothing
to be as heavily advertised as books or movies. Since women's clothing
styles change more often than men's styles, it is not surprising that
women's clothing is more heavily advertised than men's clothing. Cloth-
ing fashions are reported in newspapers and magazines in greater vol-
ume than is true of books or motion pictures. These factors reduce the
need of sellers to advertise clothing. However, not all articles of cloth-
ing are advertised to the same extent. A possible explanation lies in the
way we buy clothing. As assurance of quality, a buyer may prefer a
well-known brand. However, if a garment fits well and pleases the
buyer, then the assurance of the salesperson and the retailer's reputa-
tion outweighs the importance of the maker's brand. Thus clothing
subject to style obsolescence purchased without being tried on is likely
to be more heavily advertised than articles tried on before purchase.
Hence shirts, blouses, and men's and women's undergarments are more
heavily advertised than men's and women's outergarments.

Closely related to these arguments is the effect of consumer knowl-
edge about the product on its advertising intensity. Manufacturer and
retailer advertising is directed at consumers before they enter the point
of purchase. Consumers also learn about goods at the point of purchase
and, often more important, ascertain the quality of goods after pur-
chase if that is possible. Consumers are most knowledgeable about well-
established products they buy frequently and many such products are
little advertised. The extent of consumer knowledge about products
should not be exaggerated since advertising can play an important role
in making consumers feel they have made a defensible choice particu-
larly when actual use of the product does not reveal its quality. For
example, few motorists know how good the gasoline they are using is
and they may be comforted by the advertising of mysterious ingredi-
ents.

Even a well-established product may be a "black box" to consumers
and, therefore, heavily advertised. Thus bakery, dairy, and meat
products are not as heavily advertised as watches, television sets, and
cameras. Though the inner workings of automobiles are as mysterious
to consumers as the latter, they are not heavily advertised relative to
sales. However, no other product is as visible to consumers as automo-
biles which advertise themselves by their presence on the street. In
addition, the absolute amount of automobile advertising is very large
and few consumers are unaware of the nuances of auto makes.

To be sure, advertisements rarely contain much technical informa-
tion about products. However, brand names signal the seller's reputa-
tion and buyers are often guided to the purchase of famous brands on
the not indefensible belief that what is purchased by many cannot be

entirely unsatisfactory. Because buyers of industrial goods are better informed about the quality of these goods than are household buyers, some see in this an explanation of the more intensive advertising of consumer goods. However true this may be, because fewer buyers typically purchase an industrial good than most consumer products, advertising may be a more efficient way of informing household buyers about the goods of interest to them than is true of the industrial goods.

These considerations lead us to return to the opening theme of this paper that advertising is only part of the resources engaged in making consumers aware of commodities and brands. Commodities differ much less with respect to total selling costs than with respect to advertising for the same reasons that they differ much less with respect to total advertising intensity than with respect to particular kinds of advertising expenditures.

SCALE, SPECIALIZATION, AND COSTS IN RETAILING*

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One often hears the practitioners in the field of marketing argue that distribution has been increasing in efficiency over the years because o: "mass distribution"; i.e., because, among other things, retail establishments and retailing firms are becoming larger. What are the prospects that the distribution costs in the United States might be lowered in the coming years because of the increases in the scale of operation in retailing?

As the population and the income of a country grow, we would expect the distribution network of that country to be more fully utilized. *Ceteris paribus*, if a country is rather sparsely settled and income per capita is at a given level, some minimum distribution plant is needed to service the economy. That same country with four times as large a population at the given income level would supposedly not need four times as many retail stores, wholesale establishments, and so on. Establishments in the distributive sector could be operated at rates of output closer to short-run capacity, and the size of the firm and the size of the plant could in more instances take advantage of possible economies of scale. (I have just used the term "short-run capacity." Capacity in retailing is a very awkward concept. Let us say that if the store is operating at a point where unit costs are still falling, it is still operating short of capacity.) Thus in the more densely populated economy, distribution costs per unit of goods moved would be lower. If the greater population is associated with greater income per capita, then the volume of goods moving through the distributive sector would be still larger, relative to the low-population country, and there would be more reason to expect economies resulting from a more fully-utilized distribution plant.

In this paper I wish to discuss this question by first commenting on certain unique aspects of the scale problem in retailing and then by reviewing some preliminary findings of a study of the 1954 Census of Business data on retailing. Certain implications for the question of distribution costs in the United States can then be cited.

"Product" and "Scale" in Retailing

One reason retailing is rather "messy" for economists to deal with

conceptually is that the nature of the product is a bit peculiar. Normally we think of the retailer's products as being the goods on the shelves. This view is reinforced by the widely-accepted classification scheme employed by the Census, in which retail establishments are classified mostly by the major type of product sold. But clearly a distinction must be drawn between the physical goods and the services which the retailer adds to those goods—services which are paid for with his dollars of gross margin. These services include not only the credit and delivery services but also such features of his "product" (in the broad sense) as the furnishings of the store, the breadth of selection, service on customers' complaints and, of course, his location. These services constitute the major bases for product differentiation among retailers. Let us consider, therefore, that the retailer's product consists of the physical goods he stocks plus what we will call the service component of the total "product" of the store.

Strictly speaking, the nature and quality of this service component are not independent of the scale of the establishment. Unlike most other firms, the retailer cannot move out along his long-run cost function without changing the nature of his product, whereas the garden variety cost function we customarily employ in economic analysis assumes that the product is given. Regardless of whether scale is measured by sales, assets, inventory, floor space, or employees, the larger scale store is not just a larger store but a different one in the eyes of the consumer. Even if the product lines are identical in the two stores, in the larger store the floor space would be greater or the customers more crowded; thus causing the product to be different in the larger store. In practice, of course, the larger store offers a larger selection of goods to help offset the greater searching costs which the consumer must incur.

Besides the interdependence of quality of product and scale of establishment, retailing is also unique in one critical aspect of the buyer-seller relationship. If we ignore mail and telephone orders, we can say that retailing as a form of production is singular in that the consumer must visit the plant in order to buy the product. The retailer, then, if he is to expand his sales, must at some point begin offering enough inducements to attract trade, not only from nearby competitors, but from more distant competition as well. These inducements may be in the form of lower prices or greater services of various sorts. But because the consumer is so often interested in visiting more than one retail store in a given trading center, the market radius for the individual retailer is clearly dependent not only on the nature of his own offerings but on the number and the nature of the offerings of the retailers clustered around him.

This interdependence of location in retailing complicates the discus-

sion of market areas in retailing as compared with manufacturing or primary production. In the latter two cases, we are accustomed to thinking of the case of two competitors located some distance from each other; the market lying between the two firms is divided at the point where the transportation cost gradients extending out from the two firms intersect. In retailing, however, the more relevant transportation cost is not that on the goods themselves, although this is obviously involved when the retailer extends delivery service, but rather the cost of transporting the consumer from his residence, in most cases, to the cluster of retail stores. The retailer, or the group of retailers, must absorb the freight, not on the goods, but on the customer; i.e., they must offer goods of lower price, of better quality, or in greater breadth of selection by such a margin that the buyer is compensated for the extra effort involved in coming to the given retailer or retailers rather than to any others.

Increases in the scale of establishment in retailing, then, are associated with changes in the nature of the product offering, not only because a bigger store is a different store in the eyes of the consumer even if the physical goods offered were identical, but also because the larger store in practice so often differentiates itself by means of non-price inducements to consumers. Since these non-price services, including the breadth of selection, so often involve some costs, the economies of scale which might obtain if the product were held constant do not in fact appear to the extent that they otherwise might.

The large-scale establishment is also thwarted because the greater the market to which it sells, the more conducive is the environment to specialty shops which can bite off segments of the large store's market. The large store may offer a complex of goods and services which appeals to the typical consumer; but as the market grows, the number of atypical consumers can reach the point where new entrants can survive by segmenting the market. In the department store case, the large store attempts to segment the market itself by establishing a budget shop and a bargain basement, for example, as well as special departments catering to the high-income trade. But the point remains that market segmentation by new entrants can chip away at the sales of the larger store; thus restricting the scale of the large establishment.

To summarize to this point, the scale of establishment in retailing, mail and telephone orders aside, is limited because the customer must visit the plant. Therefore, large scale is associated with a different product mix, and it is by no means clear that this larger, but different, product mix permits lower unit costs of distribution if these costs are defined as the costs of operating the retail store. Furthermore, as the store increases its market radius, it increases the probability that new

stores will enter to specialize in offering a bundle of goods and services which diverts a segment of the market from the large establishment.

Trends in the Scale of Establishment in Retailing

Without taking the time for a detailed review of the few available studies of cost functions in retailing, we can say, to the extent that generalizations are justified in this area, that the long-run average unit cost function in retailing drops off precipitously at quite low rates of output but declines only gradually over a rather wide range and may eventually turn up again. In other words, the function has the shape of a reversed J, with the nearly horizontal reaches of the function covering quite a wide range of output. If we had detailed cost studies available for each kind of business in retailing, we could determine roughly whether the existing establishments are generally operating at low-cost rates of output or whether a substantial proportion of the existing establishments in the kind of business were still operating at rates of output associated with high unit costs. We do know that in 1954 about 30 per cent of the total retail trade in consumer goods (exclusive of non-store retailers) was accounted for by establishments in rural areas and in cities of less than 10,000 population. Furthermore, sales per establishment in these places are only one-third to two-thirds as great as in the rest of the country. It may be that a substantial portion of this 30 per cent of retail trade is from establishments operating well short of the lowest portion of the long-run average unit cost curve. However, some of these rural areas in the Census are really unincorporated parts of metropolitan areas.

Between 1939 and 1954, the sales per establishment in U.S. retailing increased very substantially. After arranging consumer goods retail trade into forty kinds of business in a classification scheme which permits comparisons of 1939 and 1954, one finds that the mean increase in real sales per establishment is about 111 per cent. (The deflators used were tailored to the individual kinds of business because a measure of the increase in physical volume per establishment was sought; but the indexes leave much to be desired.) Of the forty kinds of business, only six showed increases greater than the weighted mean. Among these six, of course, were some of the kinds of business which account for a high proportion of retail trade; namely, gasoline service stations, grocery stores, and both franchised and unfranchised passenger car dealers. The median increase was roughly 80 per cent, compared with the mean of 111 per cent. Department stores, which account for about 7 per cent of consumer goods retailing, nearly doubled in real sales per establishment over the same period.

The six kinds of business which pulled the mean increase up so high

have seen their share of the total in-store consumer goods retail trade increase from about 40 per cent in 1939 to over 50 per cent in 1954, while the six kinds of business with the smallest increases in sales per establishment suffered a slight drop in their share of the total retail market. Thus retail trade seems to be gravitating toward the kinds of business which have experienced the greatest increases in sales per establishment. (I do not intend to imply any cause-and-effect relationship here, but only to state the situation.) Furthermore, examination of the preliminary 1958 Census data suggests that this trend is continuing.

The point might be put another way: The four kinds of business which account for the largest proportions of in-store consumer goods retailing are grocery stores, franchised passenger car dealers, gasoline service stations, and department stores, in that order. These four are all among the top eight in our list of forty kinds of business mentioned above. These four kinds of business accounted for 50 per cent of in-store consumer goods retail sales in 1939, 53 per cent in 1954, and about 57 per cent in 1958.

From this, then, it is apparent that in order to say much about the prospects for distribution costs in the United States, we must learn not only what the cost functions for the individual kinds of business look like so we can tell what happens in that kind of business as sales per establishment increase; but we also need to know how the cost functions for the various kinds of business compare with one another, since the market shares of the various kinds of business are shifting significantly over time.

Multiple regression analysis of the sales per establishment data for the Standard Metropolitan Areas in 1954 suggests that the SMA's which are growing fastest have the largest sales per establishment among the convenience goods, but that a fast rate of growth is not associated with large establishments in shopping goods, passenger car dealers excepted. Furthermore, high-income SMA's have larger establishments in rather few kinds of business. As a matter of fact, women's clothing and specialty shops decrease in size with increases in income; i.e., they are smaller in the high-income cities.

It may be that sales per establishment will continue to increase among the convenience goods, but that the rate of increase in apparel stores and many of the other shopping goods stores will be much slower. This would seem to be a likely result of the suburbanization of retailing.

Scale of Firm in Retailing

The phenomenon of "mass distribution" presumably involves goods moving not only through large retail stores but also through large re-

tail firms. How have the chain stores been faring in recent years? The data are handled most simply if we talk of single-unit versus multiunit firms. The single-unit establishments since 1929 have held their share of total in-store consumer goods retail trade between 65 and 71 per cent, contrary to the common impression that chain stores are expanding their share of the market all the while. The advance report for 1958 indicates that the single units still held 71 per cent of the market in that year.

But the percentage of sales accounted for by single-unit establishments in 1954 ranged all the way from 97 per cent for drinking places and 95 for franchised passenger car dealers down to 17 per cent for variety stores. One can ask whether chain organizations will not spring up in those kinds of retailing where single-unit firms are now so strong. I have explored this question in another paper and so I will merely summarize the conclusions here. Chain store organizations have an advantage in those types of retailing in which substantial economies can be gained from reducing to a routine the decision making at many points in the distribution process. In other words, the distribution of goods in the mass is a means of reducing the cost of decision making per dollar of sales. Integration is slow to develop in some types of retailing because decision making cannot readily be reduced to a routine; because the economies so gained are not significant relative to the total cost of doing business; or because various means of quasi-integration produce the desired economies.

In the future, if means of reducing to a routine the decision making in those kinds of business in which single-unit establishments are now strong, will distribution costs fall? This depends on whether the savings from the routinization are passed on as price reductions or whether the firms concerned think that demand is less responsive to price reductions than to other components of the "marketing mix." If the savings from routinization are spent on selling costs, we would not necessarily expect the expansion of the multiunit retailers to result in lower distribution costs.

Implications for Distribution Costs

The data show that the scale of establishment in retailing increased markedly between 1939 and 1954. Increases between 1954 and 1958 are also generally evident despite the fact that 1958 consumption expenditures were below the trend line. Not only are the establishments in each kind of business increasing in size but sales are shifting from the kinds of business in which establishments are small to kinds of business in which establishments are larger. Although our knowledge of cost functions in retailing is fragmentary and sales per establishment

is a poor measure of scale, it seems probable that retailing in the U.S. is operating with less excess capacity and closer to the long run least cost output now, than in the past.

Looking at the firm rather than the establishment, we can say that multiunit firms in retailing seem not to be increasing their share of the market, which would suggest that the major economies resulting from integration in retailing have already been achieved.

Even if increases in the size of the establishment or of the firm in retailing bring lower unit costs in terms of the usual cost function, the cost savings may be used by the retailer not to reduce prices to consumers but rather to increase other components of the marketing mix. So it is not at all clear that the increases in scale of establishment or of firm will operate to lower prices to consumers, although it may well be that greater retailing services per dollar of value are almost inevitable.

AN INTERPRETATION OF CHANGES IN AGRICULTURAL
MARKETING COSTS

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In the twenties and again in the fifties, American agriculture suffered from "cost-price squeezes." Indirectly, these squeezes were expensive to the whole economy—to nonfarmers as well as to farmers.

In 1928, Warren and Pearson [7] wrote: "The maladjustment between farm and retail prices has been the most serious single factor in causing the agricultural depression." Whether this was correct or not, increased food marketing costs during World War I and the relative inflexibility of these costs in the twenties doubtless contributed significantly to the depressed condition of American agriculture in the twenties. This situation led to agitation for "farm relief," which in turn led to the big expensive farm programs we have today.

The trends of the fifties were different from those of the twenties. In the fifties, retail food prices moved slowly upward. But food marketing costs rose more. So the farmer got less. As a result, farm income fell in the fifties, while nonfarm income reached unprecedented highs. Rising marketing costs were obviously an important factor in the squeeze of the fifties. That squeeze has resulted in further expansion of the government's farm program. And the squeeze is not yet over.

We propose to review some of the major trends in food prices and marketing costs since 1913, to discuss some of the reasons for the cost-price squeeze of the fifties, and to suggest some of its implications to farmers, to consumers, and to taxpayers.

In this review and analysis we shall use the broadest possible concept of marketing costs.[1] We shall include all costs incurred as the farm product moves through the market to the final consumer; also the costs of nonfarm services to the farmer. In short, we shall include all nonfarm costs of food. These are the costs incurred in the markets for food and in the markets for farm supplies. They include costs of processing and storing food, for example. Also, they include the cost of such nonfarm services as those for farm machinery and the processing of feeds.

We have no quarrel with those who may prefer a less inclusive definition. But for the purpose of this paper, we are concerned with all these nonfarm costs, whatever they may be called. We are concerned, in other words, with the difference between the cost of food to the consumer and the net amount of money retained by the farmer. It is this

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[1] For a discussion of the rationale of this broad concept see Trelogan and Ogren [6].

difference—or spread—that has been increasing in the fifties. And the change in this spread has been an important factor (but certainly not the only factor) in the drop in farm income while nonfarm incomes were rising. We find it convenient to call the total spread between consumer costs of food and gross returns to farmers the "marketing cost" or the "marketing bill." The costs of the nonfarm services to the farmer that are a part of his gross returns we call the "farmer's bill for nonagricultural services."

We shall discuss the trends for food only. This is because we have no time series of the marketing costs for textile and tobacco products.

Trends Shown by Market Basket Data

For many years the U.S. Department of Agriculture has computed the annual retail store cost of a typical family market basket of food, and has compared this cost with the income the farmer got for the farm products used in the same foods [5], Part II. The difference is called the marketing spread, or marketing cost. For example, in 1960, the market basket cost the city consumer $1,045; the farmer got $400; so the marketing spread was $645.

So far as possible, the Department prices the same basket of foods each year; that is, the same quantities and the same qualities of the same foods. But, of course, changes have to be made in the market basket from time to time to keep it up to date. For example, the market basket now includes frozen concentrated orange juice and other frozen and prepared foods that were unknown twenty years ago.

To show long-term trends in the market basket data, we have spliced several series together, as is commonly done with index numbers. The result, shown in Figure 1, is an estimate of what a comparable market basket of farm food products would have cost consumers each year since 1913, the farm value of the equivalent products, and the marketing spread.

Look first at the period from the beginning of World War I (1914) through 1929. Inflation and rising incomes during the war pushed up the retail store cost of the market basket, the value to farmers, and the marketing spread. The marketing spread increased more than the farm value of the foods. Then the marketing spread remained at about 80 per cent above the prewar level. When retail food prices fell after the war and when marketing costs were almost inflexible, the only thing that could give very much was the price to farmers. The marketing spread did drop in the severe price deflation of 1920-21 but remained well above the peak reached during the war period.

Everything had to give in the big depression of the thirties. Consumers paid less for their market basket of food; the middleman took

less; and the farmer got less for producing the raw materials used in the foods.

World War II again brought higher incomes and general inflation. In spite of the OPA, the cost of the market basket rose sharply, along with all other prices, wages, and profits. During the war, and until 1948, the gain in the farm value was greater than the increase in the marketing spread. Prices received by farmers went above the 1910-14

THE MARKET BASKET OF FARM FOODS*

DOLLARS

Retail-store cost

Marketing spread

800

400

Farm value

0

1913   1923   1933   1943   1953   1963

*DATA FOR 1947-60 BASED ON AVERAGE 1952 PURCHASES BY URBAN FAMILIES.
DOLLAR FIGURES FOR 1913-46 ADJUSTED FOR DIFFERENCES IN COMPOSITION.

U. S. DEPARTMENT OF AGRICULTURE   NEG. 8231-60 (11)   AGRICULTURAL MARKETING SERVICE
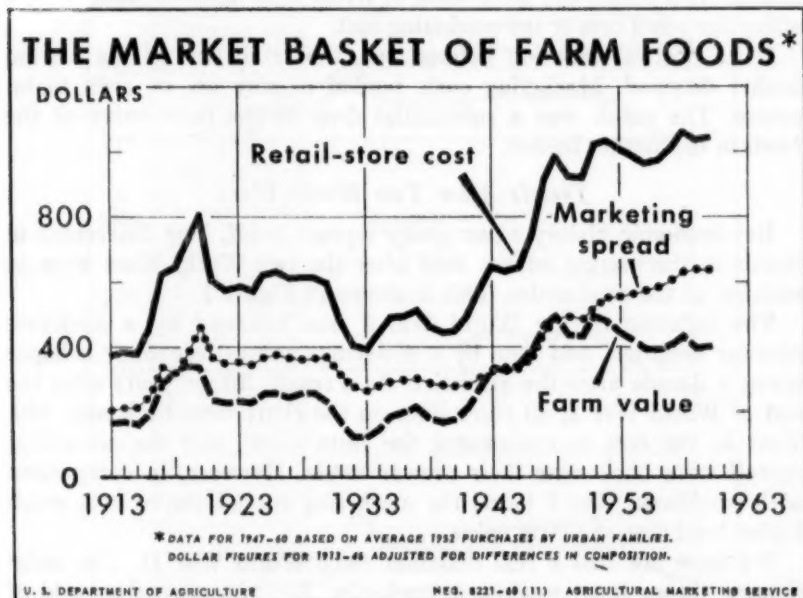
FIGURE 1

parity levels that Congress had designated as the basis for price supports.

The recession in 1949 brought a taste of what was to come in the fifties. In 1949 and 1950, the cost to the consumer and the returns to the farmer dropped, while the marketing spread stayed approximately unchanged. The Korean war brought a temporary spurt to all three components.

In the fifties, there was no definite trend in the cost of the food basket to consumers, but the cost did rise 14 per cent from 1950 to 1960. After 1951 the farm value went down in most years, but the marketing spread increased in each year for a total rise of 32 per cent. It is this continued rise in the marketing spread since 1950 that has serious implications to farm income and to the size and cost of farm programs.

The increasing marketing spread in the fifties which is shown in Figure 1 was not due to more marketing services. Figure 1 is based upon the market basket data, which, so far as possible, prices foods of the same kind and with the same services from year to year.

Briefly, Figure 1 demonstrates the following general tendencies:

1. During both wars and both postwar inflations the retail cost of the market basket rose. For a time, marketing costs increased more slowly. As a result, the farm value of foods went up more sharply than either the retail cost or the marketing cost.

2. As postwar inflation petered out, the retail cost of the market basket dropped. Marketing costs tended to stay up, or even to increase. The result was a substantial drop in the farm value of the foods in the market basket.

Trends After Two World Wars

But economic history never really repeats itself. The differences in trends in the market basket data after the two World Wars were as striking as the similarities. This is shown in Figure 2.

The inflation during World War I was followed by a moderate postwar deflation, and then by a disastrous general economic collapse about a decade after the armistice. As a result, fifteen years after the end of World War I, all three lines on the chart were back near 100. That is, the cost to consumers, the farm value, and the marketing spread were back near their prewar levels. However, in comparison with pre-World War I levels the marketing spread stayed at a much higher level than the farm value.

We have not had a real deflation since World War II. The inflationary forces have weakened gradually. But the general trends of most prices, costs, and incomes have been upward since the end of World War II. As a result, all three lines are still substantially above their prewar levels. Even the farm value of the foods in 1960 was more than double that of 1939. These increases were largely the result of changes in costs, due in turn to a large drop in the purchasing power of the dollar.

In both postwar periods, the marketing spread increased relative to the farm value, and thus contributed to the cost-price squeeze in the twenties and again in the fifties.

Food Expenditures and Their Breakdown

Actually, of course, consumers are not buying the same basket of food that they purchased twenty or thirty years ago. They are buying foods that are more highly processed and sold in fancy packages. They are buying more food in restaurants, and more of their food is pur-

chased and less comes from farm and home gardens. Thus, the total amount of money spent by consumers for food has gone up more than has the cost of a standard market basket.

THE MARKET BASKET: TRENDS DURING AND AFTER TWO WORLD WARS
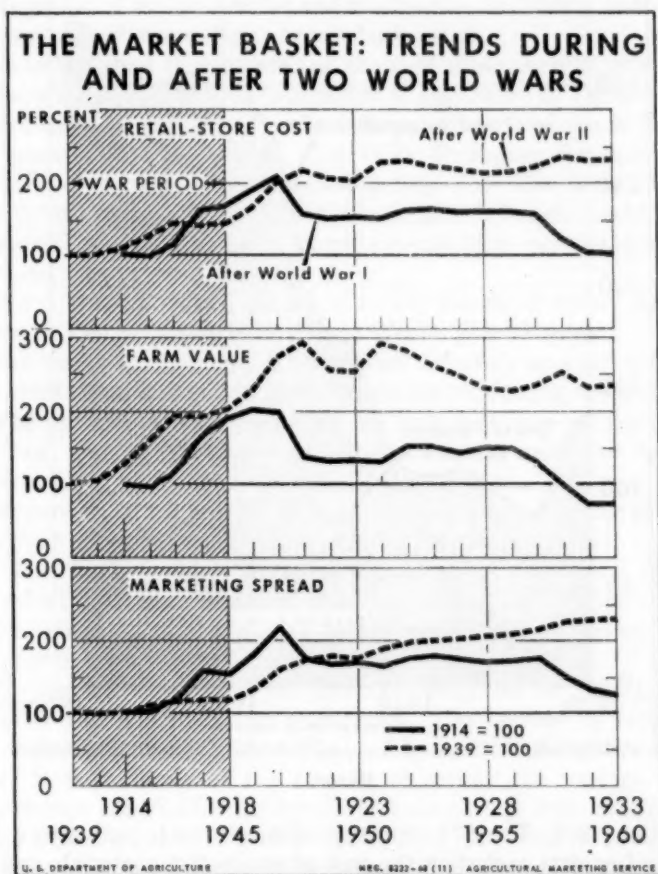
FIGURE 2

Figure 3 shows the trend in per capita food expenditures from 1929 to 1959. It also shows how much of the total expenditures for farm foods (excluding imported foods and seafood) were absorbed by the food marketing bill and by the farmer's bill for nonagricultural services. The chart shows, for example, that in 1959 per capita expenditures for farm food were $332; the food marketing bill absorbed $220; leaving gross returns to farmers of $112; of this, the farmer spent for nonagricultural services $41; leaving for agriculture (divided by U. S.

FOOD EXPENDITURES AND ITS BREAKDOWN

$ PER CAPITA*

Food expenditures

FOOD MARKETING BILL

Gross returns to farmers

FARMER'S BILL FOR NONAGRICULTURAL SERVICES

RETAINED IN AGRICULTURE

1930 1940 1950 1960

*ALL DATA DIVIDED BY THE U. S. POPULATION

U. S. DEPARTMENT OF AGRICULTURE   NEG. 8223-60 (11)   AGRICULTURAL MARKETING SERVICE
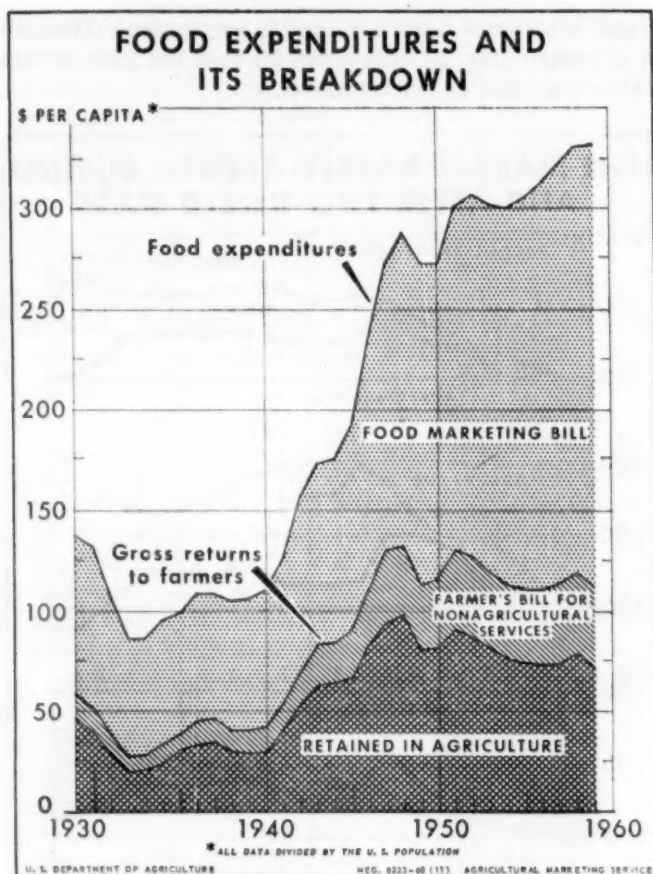
FIGURE 3

population) $71. The $71 represents the returns to agriculture for its labor and capital excluding the cost of production materials and other services furnished by the nonagricultural part of our economy. It is analogous to the value-added concept used by the U.S. Bureau of Census for many years in its periodic Census of Manufactures.

The data in back of Figure 3 are shown in Tables 1 and 2 of the appendix to this paper. Here we need to describe briefly the data on the farmer's bill for nonagricultural services.[2] It is an estimate of what the farmer spent for nonagricultural services related to food production alone—not including the expenses for producing nonfood products

[2] The data were prepared by Robert H. Masucci and Mardy Myers, with the assistance of Marguerite C. Burk, all of the Agricultural Marketing Service.

such as cotton and tobacco. The cost of these nonagricultural services was derived from total farm expenditures for feeds and seeds purchased (less cash farm receipts from sale of feeds and seeds), fertilizer and lime, operation of motor vehicles and machinery, electricity, containers, and other miscellaneous items of nonfarm origin.

The method used to estimate the share of these current production expenses attributable to food production is illustrated in Table 3 of the appendix for the year 1954. Similar computations were made for four other years: 1929, 1939, 1949, and 1959. Estimates for intervening years were based on data from two series: (1) The farm value of domestic farm food sold to U.S. civilians, and (2) cash receipts from farm marketings plus value of home consumption with an inventory adjustment illustrated in Table 3.

Figure 3 throws more light upon recent trends in actual food expenditures. The principal trends since World War II were:

1. Per capita food expenditures trended definitely upward, reflecting higher retail food prices and more expensive marketing services.

2. The per capita food marketing bill rose even more sharply.

3. Thus, although the average consumer spent more for food, he contributed less to the gross income of the farmer.

4. Moreover, of each dollar of gross income for producing food, the farmer had to pay out more and more for nonfarm services.

5. Thus, the increasing per capita payments of consumers for food were reflecting higher costs of food marketing and higher costs of nonfarm services, resulting in a rather substantial downward trend in the amount available for factors used in agriculture.

Note that all these data are averages obtained by dividing total figures by the United States population. The decline in returns to farmers shown at the bottom of Figure 3 is not in terms of the average farmer. It is in terms of the amount of money the average United States citizen contributes indirectly to farmers—or how much of the average citizen's food bill goes to the farmer. The returns of the average farmer dropped less than this because the farm population fell by 5 million persons from 1949 to 1959, while total population rose by 28 million.

We do not propose here to discuss the difficult problem of measuring trends in the well-being of the average farmer, nor to compare the average farm income with nonfarmers. But it seems clear to us that the postwar trends, shown to the right side of Figure 3, were distinctly unfavorable to agriculture.

Our discussion of the trends in Figure 3 implies a theory of price-and-income determination. It implies (we think, correctly) that, in the long run, the consumer's food expenditures are determined by certain

market forces; that gross returns to farmers are what is left of these expenditures after marketing costs are paid; and that returns to farmers for their labor and capital are what is left of gross returns after the farmer pays for nonagricultural services. In other words, it implies that prices and incomes are determined basically at the consumer level and that returns to the farmer are a residual. Some may argue the other way around: that farm income is determined by basic economic forces and that the consumer has to pay this plus all the marketing costs.

This is an ancient controversy. We cannot settle it fully here. But it seems to us that the weight of both economic theory and statistical evidence tends to support our interpretation of the data. Economic theory would indicate that consumer expenditures are determined by their preferences (technically, by their indifference functions), by their incomes, and by the amounts of goods available. Again, economic theory would suggest that marketing costs and the costs of nonfarm services are determined by such things as the amounts of services required, by wage rates, and by other cost rates.

These theoretical hypotheses have been supported by the findings of statistical studies made by Been [1], Bunkers and Cochrane [2], Burk [3], and Daly [4].

In an earlier study, based on data for 1913-44, Been noted that "the relative stability of marketing margins in comparison with the variation of retail prices is a matter of great importance in determining the influence of the food-marketing-margin structure upon prices and incomes received by farmers" [1, page 22].

In the three later studies, all authors stressed the growing importance of food marketing services, the much greater income elasticity in demand for marketing services than in demand for food products at the farm level, and the implications of these findings on food and agricultural industries. Bunkers and Cochrane estimated the elasticity of farm food product consumption with respect to income in the range from .25 to .28 and of marketing services with respect to income from .96 to 1.32 [2, page 215]. These estimates were computed from data for 1913-54. Burk concluded that the level of use of food marketing services rose substantially from pre-World War II to post-World War II, with much of the change occurring in 1939-41 and 1945-47 [3].

Some Statistical Relationships

The statistical studies mentioned above have thrown a great deal of light upon the forces that have determined food expenditures and the marketing bill. But we thought it desirable to try a modest addi-

tion to existing information along two lines: first, by using the new data on the farmer's bill for nonfarm services; second, by comparing relationships in postwar 1948-59 with those in prewar 1929-40.

Originally, we planned to show for each period how food expenditures, the marketing bill, and the farmer's bill for nonfarm services were related both to consumer's income and to domestic food marketings. But the regressions indicating the effects of domestic marketings on both the marketing bill and on the farmer's bill for nonfarm services were statistically nonsignificant. Apparently, year-to-year changes in volume did not significantly affect year-to-year changes in marketing costs or in costs of nonfarm services. We decided, therefore, to use the following equations. (All data are per capita and are taken from Table 2.)

Equation Number		Squared Correlation
	For 1929–40	
1-a	Food expenditures $= 90.864+0.176$ income -0.878 marketings $\quad\quad(0.017)\quad\quad\quad(0.451)$	$R^2=0.935$
2-a	Marketing bill $= 32.764+0.066$ income $\quad\quad(0.013)$	$r^2=0.727$
3-a	Nonfarm services $= 1.129+0.018$ income $\quad\quad(0.004)$	$r^2=0.726$
4-a	Gross returns to farmer $= 58.100+0.110$ income -0.878 marketings	
5-a	Net returns to farmer $= 56.971+0.092$ income -0.878 marketings	
	For 1948–59	
1-b	Food expenditures $=386.593+0.139$ income -2.917 marketings $\quad\quad(0.018)\quad\quad\quad(0.899)$	$R^2=0.947$
2-b	Marketing bill $= 21.819+0.103$ income $\quad\quad(0.005)$	$r^2=0.979$
3-b	Nonfarm services $= 20.638+0.011$ income $\quad\quad(0.002)$	$r^2=0.698$
4-b	Gross returns to farmer $=364.774+0.036$ income -2.917 marketings	
5-b	Net returns to farmer $=344.136+0.025$ income -2.917 marketings	
	The numbers enclosed in parentheses are standard errors of the regression coefficients directly above them.	

Equations 1-*a*, 2-*a*, 3-*a*, 1-*b*, 2-*b*, and 3-*b* are least-squares regression equations with the variable shown to the left treated as dependent. Equations 4-*a*, 5-*a*, 4-*b*, and 5-*b* were "derived." Thus, equation 4-*a* is equation 1-*a* minus equation 2-*a*. To save space, we can write this

as $4\text{-}a = 1\text{-}a$ less $2\text{-}a$. Similarly, $5\text{-}a = 1\text{-}a$ less $2\text{-}a$ less $3\text{-}a$; $4\text{-}b = 1\text{-}b$ less $2\text{-}b$; and $5\text{-}b = 1\text{-}b$ less $2\text{-}b$ less $3\text{-}b$. This is in line with the theory of derived demand and derived income described above.

Comparing the demand equations, $1\text{-}a$ with $1\text{-}b$, the coefficients indicate that before World War II, 17.6 cents out of each added dollar of consumer income was spent for food, and that this dropped to 13.9 cents after the war. On the other hand, comparison of the prewar and postwar equations indicate that an increase of one point in the index of domestic marketings reduced consumer expeditures by 87.8 cents before the war, and by $2.917 in the postwar period. Evidently in the postwar years, consumer food expenditures responded less to increased incomes and more to increased marketings than they did before the war. Note that marketings increased at a faster rate than food consumption because of an increase in the proportion of the food supply moving through the marketing system.

These regression coefficients are not elasticities. We think that for most practical purposes they are more meaningful and useful than elasticities. But economists have become so addicted to coefficients of elasticity that we felt almost compelled to show elasticities here. To compute elasticities, we need the following data:

	Prewar 1929–40	Postwar 1948–59
Mean of per capita food expenditures.................	107	304
Mean of per capita consumer income.................	509	1,585
Mean of index of per capita domestic marketings.......	83.25	103.8
Effect of one additional dollar of income..............	+ 0.176	+ 0.139
Effect of one additiona lindex point of marketings......	− 0.878	− 2.917

From these data, we can compute two kinds of elasticities: the partial elasticities of food expenditures with respect to consumer income (with domestic marketings held constant) were 0.837 in the prewar period and 0.725 in the postwar period; the partial elasticities of food expenditures with respect to domestic marketings (with consumer incomes held constant) were −0.683 in the prewar period and −0.996 in the postwar period.

Implications

So much for elasticities. Returning to the regression equations, in both periods, increases in consumer income tended to raise food expenditures. In the postwar period the rise in food expenditures would have been much greater had it not been for increased domestic food marketings. If marketings had been held constant in 1929 to 1940, each added

dollar of consumer income would have meant 17.6 cents more spent for food. Of this, 6.6 cents would have been absorbed by higher marketing costs, and 1.8 cents would have been absorbed by higher payments for nonfarm services. The addition to returns to agriculture for its labor and capital would have been $17.6 - 6.6 - 1.8 = 9.2$ cents.

In the years 1948-59, the comparable returns to agriculture would have been $13.9 - 10.3 - 1.1 = 2.5$ cents added returns to agriculture from each extra dollar of consumer income.

A drop from 9.2 cents to 2.5 cents in returns for each added dollar of consumer income has serious implications to the future of American agriculture. Of course, 2.5 cents is much better than nothing at all. As the population grows, and as per capita incomes increase, returns to agriculture would rise unless the rise were offset by further increases in output and in marketings, such as those that have occurred since the war.

But if agriculture is to share adequately in further gains in national income, it will probably require one or more of the following: (1) a less rapid expansion of output and domestic marketings; (2) larger diversion programs to remove surpluses from the domestic food market; (3) larger governmental payments to farmers; and (4) a slowing up, or reversal, of the trend toward more marketing services and more expensive marketing services.

With respect to the last possibility, a contraction in marketing services and a decline in the cost of these services is not likely—unless we have another Great Depression. With rising incomes, consumers will continue to expand their purchases of food marketing services much more than their purchases of food products. Food processors and distributors have made marked gains in efficiency, especially in recent years, but increases in labor and other costs have more than offset these gains. Some further cost increases are likely. There are more rigidities built into the marketing-cost structure—on the downward side—than at any previous time.

Thus the failure of agriculture to share fully the post-World War II gains in income may not be a temporary situation. The food-marketing bill probably will increase further—at least so long as consumer incomes continue to rise. Also, farmers likely will pay a higher proportion of their gross returns for nonagricultural services as nonfarm inputs are expected to increase relative to farm inputs. Prices of these services also are becoming more rigid.

As the marketing bill becomes an even larger part of the consumer food bill, the impact of changing retail prices of food products on farm prices is intensified, unless offsetting measures are taken. For example, all of the continued increase in marketing costs for bread during the

fifties has been reflected in higher retail prices because of the price support program for wheat. In these circumstances, any program that is effective in supporting farmers' incomes will continue to be expensive, either in terms of food prices or in terms of taxes. And the failure to support farmers' incomes might cost even more by contributing to general economic instability.

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APPENDIX

TABLE 1

BREAKDOWN OF TOTAL EXPENDITURES FOR DOMESTIC FARM
FOOD PRODUCTS, 1929–59

(In Billions of Dollars)

Year	(1) Expenditures for Farm Foods	(2) Food Mar-keting Bill	(3) Gross Returns to Farmers (1–2)	(4) Farmer's Bill for Non-agricultural Services	(5) Left for Agriculture (3–4)
1929	$16.88	$ 9.66	$ 7.22	$1.51	$ 5.71
1930	16.25	9.92	6.33	1.53	4.80
1931	13.26	8.60	4.66	1.05	3.61
1932	10.91	7.51	3.40	.89	2.51
1933	10.93	7.37	3.56	.88	2.68
1934	12.12	7.85	4.27	1.29	2.98
1935	12.64	7.62	5.02	1.08	3.94
1936	13.99	8.21	5.78	1.52	4.26
1937	14.08	8.10	5.98	1.40	4.58
1938	13.59	8.39	5.20	1.31	3.89
1939	13.77	8.60	5.17	1.42	3.75
1940	14.69	9.10	5.59	1.62	3.97
1941	16.98	9.86	7.12	1.72	5.40
1942	21.04	11.72	9.32	2.11	7.21
1943	23.84	12.44	11.40	2.80	8.60
1944	24.38	12.80	11.58	2.81	8.77
1945	26.82	14.20	12.62	3.30	9.32
1946	33.46	17.81	15.65	3.87	11.78
1947	39.36	20.67	18.74	5.15	13.59
1948	42.20	22.90	19.30	5.01	14.29
1949	40.81	23.88	16.93	4.96	11.97
1950	41.46	23.87	17.59	5.13	12.46
1951	46.45	26.43	20.02	6.01	14.01
1952	48.20	28.28	19.92	6.24	13.68
1953	48.28	29.29	18.99	5.94	13.05
1954	48.80	30.37	18.43	6.00	12.43
1955	50.39	32.11	18.28	6.04	12.24
1956	52.63	33.96	18.67	6.39	12.28
1957	55.15	35.69	19.46	6.78	12.68
1958	57.66	36.98	20.68	6.89	13.79
1959	58.79	38.99	19.80	7.24	12.56

TABLE 2

Breakdown of Per Capita Expenditures for Domestic Farm
Food Products and Related Data, 1929–59

Year	(1) Expenditures for Farm Foods	(2) Food Marketing Bill	(3) Gross Returns to Farmers (1–2)	(4) Farmer's Bill for Nonagricultural Services	(5) Left for Agriculture (3–4)	(6) Food Marketings* 1947–49 = 100	(7) Consumer Disposable Income
1929........	$138	$ 79	$ 59	$12	$47	84.0%	$ 682
1930........	132	81	51	12	39	83.0	604
1931........	107	69	38	9	29	83.0	515
1932........	87	60	27	7	20	80.4	390
1933........	87	59	28	7	21	80.4	364
1934........	96	62	34	10	24	82.7	411
1935........	99	60	39	8	31	78.0	459
1936........	109	64	45	12	33	82.3	517
1937........	109	63	46	11	35	82.4	551
1938........	105	65	40	10	30	84.0	506
1939........	106	66	40	11	29	87.9	538
1940........	111	69	42	12	30	90.9	576
1941........	127	74	53	13	40	91.8	697
1942........	156	87	69	16	53	93.7	871
1943........	174	91	83	20	63	90.2	977
1944........	176	92	84	20	64	91.2	1,060
1945........	192	102	90	23	67	94.4	1,075
1946........	237	126	111	28	83	103.9	1,136
1947........	273	143	130	36	94	102.5	1,181
1948........	288	156	132	34	98	98.4	1,291
1949........	273	160	113	33	80	99.1	1,271
1950........	273	157	116	34	82	100.9	1,369
1951........	301	171	130	39	91	98.5	1,473
1952........	307	180	127	40	87	101.2	1,520
1953........	302	183	119	37	82	103.6	1,582
1954........	300	187	113	37	76	104.7	1,582
1955........	305	194	111	37	74	107.0	1,660
1956........	313	202	111	38	73	109.4	1,742
1957........	322	208	114	40	74	107.9	1,804
1958........	331	212	119	40	79	106.3	1,826
1959........	332	220	112	41	71	109.0	1,905

* Volume of marketings of farm food products for U.S. civilian consumption.

TABLE 3

Derivation of Farmer's Bill for Nonagricultural
Services for Domestic Food Marketings in 1954

(In Millions of Dollars)

I. All Commodities	II. Domestic Farm Food Sold to U. S. Civilians

A. Gross returns to agriculture

				II. Domestic Farm Food Sold to U. S. Civilians
1. Cash receipts from farm marketings..			$29,953	Farm value of domestic farm food sold to U.S. civilians. $18,400
2. Value of farm home food consumption.			1,824	
3. Less duplication in form of farm share of farmers' expenditures for:				Food marketings as per cent of total value of agricultural production:
	Total	Farm Value		
a. Feed.......	$3,906	$2,062		18,400
b. Livestock...	1,563	1,160		——— = 65.1%
c. Seed.......	542	287		28,268
			−3,509	
	$6,011	$3,509		
			$28,268	

B. Current production expenses paid to nonagricultural sector

		II.
1. Fertilizer and lime...................	$ 1,274	Items 1–5
2. Miscellaneous (excluding short-term interest)...........................	1,641	(65.1% of $6,217)..... $ 4,047
3. Repair and operation of motor vehicles and machinery.....................	2,702	
4. Repair and maintenance of service buildings and fences................	442	
5. Payments to nonagricultural sector for seed...............................	255	
Subtotal...........................	$ 6,314	
Subtotal, adjusted*..............	$ 6,217	
6. Payments to nonagricultural sector for feed and livestock...................	$ 2,247	Item 6
		(87%† of $2,212)....... 1,924
Payments to nonagricultural sector for feed and livestock, adjusted*.........	$ 2,212	Total.............. $ 5,971

* Adjustment made on basis of ratio of inventory change to sum of cash receipts and value of home consumption.
† Percentage that food marketings to U.S. civilians are of total food production.

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DISCUSSION

WARREN J. BILKEY: My mission is to discuss all three of the preceding papers. I will do this by making a few specific comments regarding Mr. Telser's thesis and by suggesting a broadened conceptual framework for analyzing the issues raised by Messrs. Waugh and Ogren and by Mr. Holton.

Mr. Telser's paper is provocative, and his proposition that an inverse relationship exists between the intensity with which a firm advertises and the price elasticity of its product seems to accord with empirical observation. In addition to his own illustrations, we may note that aggregate advertising tends to increase during periods of rising personal incomes, and vice versa; at the same time there are reasons for believing that price elasticities tend generally to decrease during such periods, and vice versa. It appears, therefore, that consumer advertising in the aggregate also conforms to Mr. Telser's thesis. Even such an apparent contradiction as the failure of public utilities to advertise as much as his logic implies they should, might be accounted for by the nature of public utility regulation.

Nevertheless, we still must approach Mr. Telser's paper with scientific caution. The reason is that his argument contains some logical leaps the justification of which is not clear. These leaps constitute limitations to his argument at its present state of development, and for the time being force us to regard Mr. Telser's analysis as being an imaginative and plausible hypothesis rather than a rigorously complete explanation of a real life phenomenon. The most obvious of these limitations are the following. First, the logic of his mathematical propositions apply strictly to only one point on a firm's price and "advertising" functions—to the particular price elasticity and advertising outlay which maximizes the firm's net revenue. Inferences regarding the functions as a whole (which were necessitated by Mr. Telser's attempt to relate his theoretical analysis to empirical observation) have required the aid of assumptions which are not too clearly spelled out and which may be questioned —such as his implicit working assumption that a firm's average revenue curve and its advertising function are essentially iso-elastic or in some sense constant throughout their entire lengths. Second, the term "marginal advertising intensity" is defined (equation 8) in a very particular way; it is not identical with the usual commercial concept of advertising outlay. For this reason Mr. Telser's theoretical propositions regarding advertising are not clearly applicable to advertising as it exists in the real world. Third, Mr. Telser's theoretical formulation rests on an explicit assumption that net profit maximation (presumably short run) is the goal of a firm. Nothing is said regarding its applicability to firms whose behavior is "satisficing" (price following, target behavior, etc.) nor of its applicability to firms whose objective is the maximization of gross rather than of net revenue. The goals of American business are a controversial question; probably not all firms have the same goals. For this reason it must be shown that Mr. Telser's propositions apply irrespective

of the firm's goals, or else his analysis must be regarded as having limited rather than general validity.

I do not wish to imply that the above three limitations invalidate Mr. Telser's thesis, but, rather, that because of them it is an incompletely developed hypothesis. In its present state, his thesis still must compete with alternative hypotheses; e.g., those which regard advertising expenditures as being directly related to the amount by which a given change in such expenditures shifts the firm's average revenue curve, irrespective of the price elasticity of that curve. I hope that Mr. Telser's interesting thesis will be worked out to the point where it can be checked more accurately with fact than is now possible.

My comments regarding the paper by Messrs. Waugh and Ogren and the paper by Mr. Holton are of an entirely different nature. Both papers utilize a static, equilibrium type of conceptual framework which derived historically from the classical theory of the stationary state. Within such a framework, analysis necessarily is made in terms of demand-supply relationships, with demand originating in the preferences and purchasing power of consumers and supply originating in production costs. In it, both demand and supply are regarded as given, and frictions tend to be regarded as interferences with pure competition. Consonant with this essentially neoclassical conceptual framework, both Messrs. Waugh and Ogren and Mr. Holton have made their analyses on the basis of given consumer preferences. Their methodology appears good, and their conclusions certainly are in harmony with what may be regarded as orthodox economic thought. This discussant is concerned only with the possibility that the approaches of both papers might be broadened by utilizing an alternative conceptual schema as a framework for analysis. This possibility is explored below.

Current advances in growth theory suggest that for many types of studies it might be fruitful to dynamize the usual static neoclassical equilibrium framework by incorporating into it the idea of leading and lagging growth sectors—á la Rostow. In such a construction market forces might be conceived of as force vectors deriving from demand and supply relationships which in turn are generated by the relation between leading and lagging growth sectors. Frictions, then, could arise from inertia (i.e., lags) as well as from monopolistic or governmental interferences. Areas of rising consumption demand would be regarded as leading sectors; areas of declining consumption would be regarded as lagging sectors. I.e., consumers as such would not be regarded as a section; instead, each one of their various disbursement categories and the industries to which it corresponds would be regarded as a sector. A leading growth sector would be an industry within which heavy investment is taking place or one in which either consumption demand, government demand, or foreign demand is rising rapidly. A lagging sector would be an industry in which little or no investment is taking place, or that is experiencing a decline in domestic consumption, in governmental purchasing, or in foreign procurement. Logically, competition should tend to be stronger in the lagging than in the leading growth sectors, because the latter are pioneering industries and require more

imagination, initiative, and ability to operate. Incomes ought, therefore, to be higher in the leading than in the lagging growth sectors. Yet this does not lead to an equilibrating of the two, for the greater ease of entry into a lagging industry means that during a recession unemployed resources tend to "back up" into the lagging sectors, thus accentuating rather than eliminating existing income differentials between leading and lagging sectors.

Our proposed theoretical framework can be dynamized further by regarding consumer preferences as being largely a derivative of existing family living patterns rather than taking them as given. To illustrate, a family that frequently entertains guests for dinner is likely to manifest substanitally different food preferences in the market than will a family that never entertains. Similarly, a family living within a larger city with easy access to public transportation for getting to work, visiting friends, shopping, etc., is likely to have a less strong preference for a car than would that same family if they lived in the country far from employment, friends, and shopping facilities. As still another illustration, this discussant has found from interview studies that working-wife families have some rather substantial expenditure differences than do nonworking-wife families in the same income brackets. (There are rather obvious differences in the living patterns of the two groups.) It was found that the working-wife families tended to have larger disbursements per family member for housing, furnishings, recreation, and, at the earlier stages of the family cycle, for savings. The nonworking-wife families tended to have larger disbursements per family member for food, clothing, transportation, insurance, and, at the later stages of the family cycle, for savings. The relationship between consumers' living patterns and their preferences tends to be dynamic in the sense that particular acquisitions often alter the purchasers' living patterns in such ways as to affect their subsequent preferences. To illustrate, purchasing a new automobile may lead a family to visit more than formerly, which in turn stimulates their desires for specific kinds of clothes. As another illustration, painting a room may stimulate a desire for new furniture.

The above sketchy outline of a growth type of conceptual framework (which in some respects is an expanded and dynamized version of neoclassical theory) now can be applied to Messrs. Waugh and Ogren's paper. Our analysis then would run something as follows. Family living patterns currently involve the use of food as a part of the family's entertainment. For such purposes flavor, interests, etc., even more than nutrition, are the food attributes desired; to a certain extent this also holds for the average family's regular meals. Furthermore, the complexity of modern family living patterns forces the housewife to place a heavy emphasis on the saving of time and energy in household activities. Convenience (both regarding its purchase and its preparation), therefore, has become an additional important attribute of food from the housewife's point of view—and all evidence suggests that this emphasis will increase rather than decrease in the future. So much for the buyer side of the picture! Now let us see how well the American farmer has adapted to it. The answer seems to be, not very well. Farmers are oriented to the mass pro-

duction of simple nutrition—to foods so traditional that they can be raised by almost anyone who calls himself a farmer. Entry into this kind of farming is so easy for so many that inadequate economic growth of the economy as a whole causes some of the nation's unemployed manpower to "back up" into agriculture, which further intensifies competition in farming. Under these circumstances the leading sectors in the food field are the processors and marketers, not the farmers. The latter are a lagging sector receiving extremely competitive (low) incomes.

Using our growth type of conceptual framework, this discussant can agree with the data presented by Messrs. Waugh and Ogren, and with the basic interpretation they have given to it. However, we now can expand upon their recommendations for improving the farmers' plight by suggesting the following: (1) Stimulate over-all economic growth in such a way that the resources now backed up into agriculture can flow more easily to other sectors, and then assist that out-flow by such aids as vocational training programs, employment counciling, and perhaps by loans and grants to facilitate moving. (2) Help agriculture to become more of a leading sector by intensifying research effort on the development of new foods and fibers which will be more attractive to consumers relative to marketing and processing services than are the present foods and fibers. To the extent that such items are difficult to raise, the farmers capable of raising them will experience less competitive incomes than at present. Farmers not able to produce them either can be assisted to change to other occupations, or be subsidized if necessary. (3) Develop techniques that will enable farmers effectively to take over certain of the food processing and marketing activities so that they can receive a bigger percentage of the consumers' food dollar than at present. (4) Devise living patterns that are well-integrated wholes and which will put greater emphasis on nutrition relative to food processing and marketing services than is true of existing family living patterns. Then try to induce families to adopt these patterns. This is a much more comprehensive proposition than mere advertising; it is a sizable jump into social engineering. Personally, I do not advocate that such a program be undertaken lightly, for many moral problems are involved. (5) Governmental interferences such as agricultural subsidies, the restriction of output, and the inhibition of entry into farming also might be attempted. However, such approaches have not proven especially appealing or successful in the past and hardly can be strongly advocated for the future.

Now let us apply our growth type of conceptual framework to Mr. Holton's paper. We again start from the observation that family living patterns seem to be growing increasingly complex. From this, two implications for retail trade follow: first, consumers' time will become more and more precious and, second, an increasing variety of goods and services will be desired for the implementation of these ever more complex living patterns. For example, certain kinds of clothing will be desired for formal wear, other types for work, other types for lounging, and various types for particular kinds of recreational activities. In each instance, specific clothes for imparting given moods will be desired. Taken together, the above two implications, *ceteris paribus*, should

lead over time to consumer pressure for having conveniently located retail establishments with a wide selection of every kind of good and service. A department store having many departments under one roof will not satisfy the above two "needs" unless a wide selection of each kind of good and service is available. Failure to provide an adequate assortment of certain goods should be conducive to the development of specialty stores. Logically, the above two needs could be met equally well by large department stores or by various specialty stores located reasonably close together. Which of these developments occurs will depend, according to our logic, on whether the large or small outlets gain the dynamic lead. However, the large outlet seemingly would have an advantage in co-ordinating various offerings, whether or not they choose to have all their departments under one roof. Mr. Holton's data seem to be in harmony with these conjectures.

In summary, I have found nothing to disagree with about these three papers. My only attempt has been to point out certain qualifications to the application of Mr. Telser's paper and to suggest that the utilization of a growth type of conceptual framework could expand the perspective of Messrs. Waugh and Ogren's paper and to some extent of Mr. Holton's paper.

REAVIS COX: Mr. Holton's description of the difficulties one faces in defining such familiar economic terms as scale and product in retailing is very helpful. I should have welcomed a fuller discussion, however, of the consequences that flow from taking establishments rather than firms as the entities whose scale is to be measured. I should also have welcomed some consideration of the significance for scale in marketing of the fact that consumption inevitably takes place on a very small scale. An individual consumes only one or two units of any product at a time. Somewhere within retailing, which is the market institution that breaks goods down into consumable units, or within consumption itself, the scale of operation has to be reduced sharply below what is economical for the preceding activities of extraction, processing, and market distribution.

When he discusses briefly the problem of defining the product of retailing, Mr. Holton has again hit on a very important matter. In my judgment he should have gone considerably further than he has, since we cannot give scale a meaningful definition unless we are clear as to what the product is. In a very real sense the retailer sells nothing but service. The physical substances he distributes are merely transferred from hand to hand down the long flow that begins with their extraction from some ore body, forest, or farm. What the retailer sells is values or utilities that he adds to the merchandise as it passes through his establishment.

These definitional matters become significant for Mr. Holton's discussion when he deals with the problem of convenience, since it is sometimes said that all the retailer does is to make it more convenient for the consumer to obtain goods and services from the economy than would be possible in his absence. An individual retailer cannot act alone in selling convenience. What the consumer needs is not convenient access to a

single commodity but what may be called "aggregate convenience"; that is, convenient access to the full range of goods and services he uses. These include not only what he buys from business enterprises but also such services as recreation, education, health, and religion. A concept such as that of aggregate convenience is essential if we are to understand fully the nature and purpose of the clustering of retail establishments to which Mr. Holton refers.

As to Mr. Holton's substantive statements and conclusions, they sound reasonable enough to me, but we need to have a much more detailed statement before we can make a final judgment concerning their validity. Whether "excess capacity" in retailing is either increasing or decreasing is very difficult to decide. What is the capacity of the retail store? Some years ago I persuaded a student to write a paper concerned with the problem, "How many filling stations are too many?" I forget the precise number of filling stations then in existence, but it was on the order of 300,000. This student found that 2,500 or 3,000 stations could be "enough," whereas, paradoxically, three or four million need not be "too many." The "correct" figure depended upon what you expected filling stations to do.

Suppose consumers were to organize their lives around access to filling stations. All they want from filling stations is the physical job of transferring gasoline from underground tanks to the tanks of automobiles. Each automobile is assigned a particular number of minutes, at a particular time, on a particular day, in a particular station. Given such conditions, a fantastically small number of filling stations would be enough. On the other hand, if consumers are to be assured that they can buy gasoline at any time of day or night without traveling more than a mile or so on any street or highway and without having to cross over to the other side of heavily traveled roads, then several million stations would be required.

Mr. Telser's paper was discussed fully by Mr. Bilkey; so I shall restrict myself to one comment. Mr. Telser started with an empirical problem. This was to explain why for any particular class of goods the percentage of the retail price spent for advertising is about the same in the United States as in the United Kingdom. To deal with this problem Mr. Telser works out a carefully detailed theoretical model. After he has finished with his equations and functions, however, and has made some hypothetical judgments as to what might be expected for various kinds of goods, he never gets back to the problem with which he started. I trust that sometime in the future he will systematically test the validity of his model against the facts with which he started.

Since Messrs. Waugh and Ogren emphasize the interpretation rather than the computation of their data, I shall make the same emphasis in considering their paper. Such an approach is justified in any event, because I believe that whereas their methodology stands up well under analysis, some of the interpretations they put on the results they have obtained are questionable.

First of all, let me speak out vigorously against the use of "marketing" to designate everything that comes between the consumer of food and the farmer. Thus defined, marketing usually includes processing, some (but not all) taxes, and a variety of other cost elements that do not really belong under the term.

So there is a danger that ordinary uncritical readers will draw seriously misleading conclusions from figures that are otherwise highly informative and useful.

I am aware, of course, that marketing men do not agree among themselves as to precisely what they mean by this term. Sometimes I feel that the only absolutely certain definition one can give of the word is that it is a strictly arbitrary combination of sounds made by the human voice or a strictly arbitrary combination of marks made on pieces of paper. Nevertheless, it would be most helpful if the agricultural economists could bring themselves to abandon a usage with which virtually all specialists in marketing wholeheartedly disagree.

Second, I believe that I detect some unconscious bias in the use these authors make of their figures. For example, much more emphasis is put upon the fact that the farmer's share of the consumer's dollar dropped in the deflations (or in the slowing down of inflation) that followed each of the world wars than upon the increases in that share during the wartime inflations. These figures are treacherous. By selecting his years carefully, one can demonstrate that the farmer's share of the consumer's dollar has increased substantially, decreased substantially, or gone through two very wide cycles and ended up about where it was half a century ago.

The fact to be explained would seem to be that the farmer's share fluctuates more violently than other shares; so that it is sometimes low, sometimes high, and rarely stable for any long sequence of years. In particular, it is necessary to see whether the various components of the spread have fluctuated uniformly or have varied a good deal among themselves in their responses to the economic situations through which we have passed.

Third, I find myself unconvinced by the apparent, though not explicitly stated, belief of the authors that there is some causal relationship between recent increases in what they call the "marketing spread" or the "marketing bill" and the woes of the farmer. Surely, the many studies of the farm problem we have had in recent years make it abundantly clear that the difficulties of agriculture as a whole or of individual farmers lie deeper than this.

Fourth, I feel that some effort must be made to state clearly what has happened to the average farmer or to particular groups of farmers if we are to use these figures properly as a guide to policy. The fact that the farming industry is a dwindling percentage of the nation's economy does not mean that individual farmers are necessarily suffering very much or that the economy as a whole is being injured. Do the authors mean to imply that we should cut back on, say, automobile production, medical services, and defense, merely because they have increased in relative importance? Alternatively, do they mean that we should do something to enlarge food consumption simply because food used to play a larger part in family budgets than it does now?

Finally, it should be pointed out that the increase in payments for nonfarm services to which the authors call attention in one of their charts means simply that the farmer has transferred part of what used to be farming to nonfarm industries. Specifically, where the farmer used to provide most if not all the energy he used in addition to his own muscle power by raising children

and animals and supporting them directly on the farm, he now buys his energy in the form of fuel and machinery from other sectors of the economy. So also with the other nonfarm services he buys. The correct statement would seem to be that farmers as a class have gone out of some aspects of the farming industry as it used to be defined and have changed the nature of farming drastically by increasing the extent of their reliance upon irrigation, fertilizers, and pesticides. To the extent that this is true, it is appropriate that the relative importance of agriculture as a sector of the economy should decrease.

What I have said should not be interpreted as denying that farmers as a class and a great many farmers as individuals have suffered severely during much of the last half century, that they accordingly have valid grievances, and that deciding how to correct these grievances is a very difficult problem indeed. Neither do I intend to deny that the data upon which this paper has been based are extremely important and useful. All that I have tried to do is to utter a few warnings about what seem to me to be misleading interpretations applied to them.

ANTITRUST PROBLEMS

THE ANTIMERGER ACT, 1950-60

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This paper attempts to sum up one aspect of ten years' experience: what has been meant in practice by "competition" and "monopoly"? Such an appraisal must be tentative. Most complaints have only been issued in the last four years. The decided cases are few and none well established as precedents. The natural place to begin is with the standards of the other antitrust laws, for the Antimerger Act simply adds a new wing to the old structure.

The economic theory of antitrust is that the competitive process prevents firms from having substantial power to determine prices by limiting production. Hence there will be a better use of resources at any given time. To maintain or increase profit, firms must seek greater efficiency or innovation; hence more progress over time.

But better resource-use through effective competition was never more than a minor object of the antitrust laws, though it has probably been an unintended major result. Legislators have never shown much interest in consumer welfare. Their chief concern has always been to protect some business firms against others, chiefly larger ones, and to prevent businessmen from being shut out of any particular market. The basic legal concept, as Mason pointed out twenty-three years ago, is not market control but exclusion [10, page 331]. Now, a competitive system economizes scarce resources by excluding the less efficient firms and methods. A monopolized market excludes any who would enter it, efficient or not. To the business concern which suffers or fears exclusion and to its legislative supporters, the distinction is without a difference.

The history of the antitrust laws in this country, therefore, shows an ebb and flow of statutes and decisions, now emphasizing competition and now protectionism in the name of "fairness" or of opposition to "economic power"—a term which nicely confuses monopoly with economies of scale. But there has also been, over the long run, "a very real development of antitrust law toward increasing emphasis on market power at the expense of monopolizing and restraining practices in Sherman Act cases" [10, pages 390-91]. The basic reason is that most transactions are between business concerns. Most attempts to fix prices

or monopolize markets hurt businessmen and therefore invite antitrust attack. Were the whole business system vertically integrated, so that all sales were to final consumers, the antitrust laws would either be repealed or would become altogether protectionist. The consumer (not to mention the national income) has enjoyed a world he never made. The dissatisfaction with the Sherman Act, which has resulted in new legislation at various times, has stemmed both from the feeling that it was not going far enough and that it was going the wrong way.

The antimerger law goes beyond the Sherman law, for it forbids certain acts where they may so much as tend to lessen competition; only the probability of the result must be shown. This distinction is real and important, but it is only one of degree. As early as 1904, Justice Holmes declared that the Sherman Act, "like many others, and like the common law in some cases, directs itself against the dangerous probability [monopoly] as well as against the completed result" [17, page 396]. Furthermore, the concept of antitrust "conspiracy" is in large measure based on probable results [13]. The important point is that competition, or monopoly, is not a brute physical fact but rather a hypothesis confirmed by the available evidence. A high level of concentration, plus price behavior very different from competitive expectations, etc., indicates, let us say, chances of 9 to 1 of effective market control. A lesser level of concentration, weaker price evidence, etc., force us to put the odds lower. The simplest and, to my knowledge, the only meaningful, distinction between the standards of the Clayton and Sherman Acts is that the former requires a lesser weight of evidence, and is therefore more severe. But the tribunal in a merger case must analyze with the same tools exactly the same kinds and types of evidence as in a Sherman Act case. It is a pathetic illusion that the market is whatever the courts choose to call it [18, page 74,659]. The market, like the weather, is simply there, whether we only talk about it or do something: apply to it the standards of Clayton, or of Sherman, or of any law, or none. This confusion between the legal standard and the economic fact is writ large in the Bethlehem Steel opinion, which designates as the geographic market in steel products the northeast quadrant of the United States, then also a three-state area within it, and then also each individual state: Ohio, Michigan, and Pennsylvania [18, page 74,681]. This sinks below error into chaos. If the northeast quadrant is a market area—is the locus of the supply-demand forces that determine the price—then the other two areas are not. The evidence that sustains any one of the three market concepts necessarily condemns the others. Until there is a retreat from Bethlehem to reason, tribunals will be able to decide according to any visceral whim, by manipulating market definitions. But this undesirable

randomizing of market definitions has also a definite protectionist bias, as will be seen.

Let us now consider the Antimerger Act criteria of competition and monopoly. The House Judiciary Committee, when reporting out the bill for passage, stated that "lessening of competition" might manifest itself, among other ways, in "an increase in relative size to the point where [the merging company's] advantage over its competitors threatens to be decisive" [4, page 8]. It had to follow, as the night the day, that complaints issued under the Act would allege simply that a merger would be a competitive advantage to the merging companies. By November, 1960, seventy-six complaints had been filed, thirty-nine by the Federal Trade Commission and thirty-seven by the Department of Justice. Of the seventy-six complaints, four may charitably be called doubtful, and fifty-two of seventy-two want the merger dissolved because the merging firms' "competitive position may be enhanced to the detriment of actual and potential competition"; or that the acquisitions have led or will lead to the diversion of trade, etc. There is a complete and unquestioning identification of competition with competitors [21]. Of course, the active search for every possible advantage (or avoidance of any disadvantage) is both cause and effect of active competition, which thus comes under direct attack in the great bulk of the complaints.

Many may contain this language as mere "boiler plate." But the very fact that some do not shows that this explanation must not be pushed too far. But of course complaints begin cases rather than end them. Let us, therefore, look at the decisions. With only two exceptions [2, 3] (in the first of which the merger charge was really subordinate anyway), every decided case has been either a consent settlement or an order of divestiture.

The horizontal elements of mergers—i.e., involving markets in which both firms may be said to have been previously active—have been treated severely and—if maintaining competition is the object—rationally. (We assume for purposes of this paper that markets have been correctly found.) The key fact has been the degree of concentration, which may overlap considerably with the market share of the combination and the fewness of competitors [1, 2, 5, 7, 9, 11, 12, 18]. The combined market share need not be strikingly high. As a rule of thumb, one could say that 15 per cent and higher may be suspect [1].

Concentration is of course not to be equated with monopoly, but serious inquiry into concentration leads directly to the nature of the product, of substitution in demand and supply, and to entry, which has been given a properly heavy weight [1, 2, 7, 11, 12, 16, 18, 20].

The tribunals have also paid attention to the character of com-

petition in the market involved. In five cases [1, 5, 9, 12, 18] they have held that competition is more than a little inhibited, so that the law should be particularly zealous to guard what is left and impose stricter standards for any merger. This use of marginal utility theory makes new law, for the holdings have *not* been that there was a monopoly or price-fixing agreement in sugar, steel, milk, or paper but only that there was a significant amount of control, not enough to violate the law against price fixing, yet a fact not to be ignored in interpreting the Antimerger Act. There is an obvious danger here of accepting too lightly superficial evidence of so-called "administered prices," a formal and meaningless category, and equating them to monopoly price. But then, every good rule can be abused. Half a dozen cases seem to have made it already clear that mergers with substantial horizontal elements, i.e., which significantly increase concentration in some markets, will not be permitted. This is a considerable achievement. The antitrust agencies are going to win and lose cases in this area, but the cases they lose will be those where the effect on concentration and competition will be adjudged insubstantial, so that the private economies need not be considered as social diseconomies.

The protectionist influence has not been needed or invoked as to horizontal elements. But it is being felt in the treatment of vertical and conglomerate elements, as must be expected. Vertical integration may reduce costs, thus yielding a competitive advantage. Vertical integration may also serve to extend market control into an adjacent stage or to increase the revenues from it. Let me state the problem in its most difficult form. Industry A sells a product to industry B at a price substantially above long-run marginal cost; output is restricted, as it must be, to keep the price up. There are now two good (if partly overlapping) profit incentives for a merger between a firm in A and a firm in B. The A company could operate at full capacity (higher level in the short run, larger scale in the long run); the B company would get its raw material at a competitive price and would not only earn a higher profit on its existing output but could *ceteris paribus* expand output and get a bigger share of the market. Depending on the particular circumstances, vertical integration would be an extension of monopoly or the circumventing of monopoly by one firm or more. For if there is enough of this integration—and it may not take much— the additional output will collapse the monopoly at the earlier stage. If the law aims at competition, then whatever the severity or laxity of one's standards, one must for any sound decision see the market structure clearly and accept the basic principle that vertical integration is at most an extender or multiplier of pre-existent market control. Any multiplier times zero is still zero.

But if the policy is protection of business concerns against loss of sales or supply, then vertical integration becomes an offense either as a competitive advantage or as "foreclosing" or "forestalling"; i.e., if after merger A no longer supplies its old customers and B no longer buys from its old suppliers. Now, absent market control, A's old customers can contact B's old suppliers, or there can be a general multilateral shift. This is simply a trifling incident; and the law is not supposed to bother with trifles. This problem has long occupied Section 3 of the Clayton Act, which deals essentially with vertical integration by contract, and policy there has zigzagged. The basic dilemma obtrudes at every turn. A contract excludes other sellers from dealing with the buyer and other buyers from dealing with the seller. Lacking an analysis of the market, there is no way of saying whether the contract merely annoys competitors because it lowers costs or decreases competition by significantly narrowing the alternatives open to other buyers and sellers. Either situation confers a "power" upon the firms involved, but only occasionally and accidentally do the tribunals explain whether the "power" is monopoly or economy.

The easy way out has received the jaw-breaking name of "quantitative substantiality" [18, footnote 51], and it means simply that when a substantial amount of dollars is involved, the contract or the merger is illegal. This is more than an error. It embodies protectionist policy in a definition by labeling a buyer or area as "a substantial market," or "a substantial segment of a market," from which it is illegal to exclude. This neatly avoids the inquiry into market structure, behavior, and results, while maintaining a deceptive appearance of it.

The attack on vertical integration per se is evident in six cases [5, 14, 15, 16, 18, 19] and decisive in two or three. (The General Motors-du Pont case [20] has the most economic sense, once the market definition is granted.) Brown Shoe [19], which forbade the merger of a shoe manufacturer with a retailer, is the most clearly protectionist. Its first main doctrine is simple-minded extrapolation: since the baby grew six inches last year, in twenty more years he will be twelve feet tall. The metaphorical language about eating a whole apple one bite at a time is based on no more than that the merger raised Brown's share of national output from 5 to 5½ per cent [19, pages 76, 111-112]. True, the court had previously defined the market other than nationally, but its neglect of its own definition, once it came to analyzing and predicting effects on the structure of the market, demonstrates that what purported to be market analysis was merely an ornament of discourse. Following the court's example, we should not take it seriously.

The second main thought is an alleged decrease in the number of shoe manufacturing plants, which if assumed true has no particular

relation with concentration or fewness of sellers, but would indicate simply increasing economies of scale. If a decrease in the number of plants is a "bad thing" in itself, we are not told.

The real meat of the decision comes in repeated statements that "independent retailers are having a harder and harder time." The reason for this sad condition is that retailers affiliated with manufacturers have "advantages in buying, selling, insurance, business planning and practices, advertising and credit arrangements," etc. [19, pages 76,110-111]—in plain English, economies of scale and of integration. Whether or not these advantages really exist, what matters is that they are the explicit reason for stopping the merger. An alleged bow to "power to control price" is soon shown to be meaningless when the court says of an increase in Brown Shoe's sales: "Such increase, *regardless of percentage amount,* gives them power. Such power not only tends to create a monopoly but substantially lessens competition by eliminating the effectiveness of the independent retailer and the smaller manufacturer" [19, page 76, 112, italics added]. It would be a wearisome task to comb out every other circumlocution. Translated into intelligible speech, it all means that the merging firms will gain economies which will make life harder for their competitors, and this must be forbidden.

We consider finally the "conglomerate" elements of a merger, the acquisition of "unrelated" facilities which can exist independently or mingled with vertical and horizontal elements. Imperfect knowledge is a sufficient but not necessary reason for such elements. When a company is available for sale, customers, suppliers, and those in the same activity—competitors or noncompetitors—are much more likely to have special knowledge of its prospects, will not need to allow so high a risk premium, and will therefore be willing to outbid those upon whom ignorance enforces caution.

In any case, acquiring additional and to some degree independent or negatively correlated sources of profits may help to insure the stability of the total corporate profit. As with hedging, whereby, e.g., a grain miller insures that he can neither gain nor lose by a wheat price fluctuation, a cotton textile firm might go into synthetics, a copper producer into aluminum, etc., expecting a gain by one product to be at the expense of the other. But a much more important case is that of additional profit centers acquired as simply the means to a larger sample, and hence greater stability of profits. Both strict hedging and diversification are summed up in the familiar formula for the sum of a variance,

$$\sigma_{a+b}^2 = \sigma_a^2 + 2r\sigma_a\sigma_b + \sigma_b^2.$$

With negative correlation, variability is much reduced; profit could be completely stabilized in the limiting case where

$$(\sigma_a^2 = \sigma_b^2, \; n_1 = n_2, \; r = -1);$$

given zero correlation (complete independence of profitability), we have in effect simply a larger sample, with a corresponding reduction in the standard error of the mean: at the limit, where the means are the same, from

$$\sigma_2 \; \text{to} \; \frac{\sigma_2}{\sqrt{2}} \; ;$$

given complete correlation, there is no stabilizing effect. To the extent that the profits of two or more activities united by merger are statistically independent, there is a conglomerate effect. The quantities of ink shed, the hairs split on what are truly "related" and "unrelated" activities, the mysteriously potent charm of "diversification" and of the power of "conglomerate size" tell us no more than this.

Profit cannot be increased but only stabilized by bringing together centers with zero or negative correlation. It is a nice question (which I will not try to answer) whether insurance by diversification may not be, like any other form of insurance, a net cost to the insured. Against the whole body of nondiversified competitors, the diversified firm may be at an actual cost disadvantage. But the doctrine of the fifty-two complaints and of Brown Shoe makes clearly illegal any possible "advantage" over individual nondiversified competitors. And if the acquired company now has better access to the capital market through the acquiring company or because of larger size, this private (usually also social) economy of scale is another "advantage." If the object of Section 7 is to protect competitors rather than competition, that should suffice.

So far, however, the conglomerate elements of a merger have not been under direct protectionist attack. The Clorox-Procter & Gamble case [12] has only some overtones. Clorox, the acquired company, made nearly half of all sales nationally and close to three-fourths in the Middle Atlantic states; competitors were very few and rivalry was largely promotion. In such markets it may pay to drive out a rival by temporary price cutting. Clorox needed no affiliation with P & G to make this profitable or to provide the modest sums needed. The market structure is decisive; the availability of funds is at most a necessary condition, and affiliation with a large company is only one of many ways of getting them. (Nothing is more "conglomerate" than the interests of a wealthy family owning an "independent" firm.) The notion that a large firm will lose money in a local operation because it can

afford to do so, puts the cart before the horse. They will do no such thing unless there is a dollar to be made eventually; and if there is a dollar to be made, a local concern will be just as keen in seeking it and much less inhibited by fear of the law.

The Examiner held that the acquiring firm, Procter & Gamble, had some real market power over retailers and could induce them to subordinate or even eliminate products competing with Clorox. If so, the acquisition was obviously not a conglomerate. The alleged "advantages" in selling, advertising, etc., which Procter & Gamble allegedly bestowed on Clorox could be viewed as a kind of limit on competition, inhibiting judicious consumer choice; but the distrust of advertising and promotion, almost axiomatic among economists, is nearly all absent from antitrust. At any rate, although Clorox could easily become another Brown Shoe at a later stage, it is not one as yet.

The fact is that a truly conglomerate merger cannot be attacked in order to maintain competition, because it has no effect on any market structure. Hence we find the Chairman of the FTC, five months after Clorox, looking to decisions on vertical integration as pointing the way:

> The acquisition by a large and powerful diversified company of a small company in a discrete industry historically shared by a number of small companies competing on equal terms followed by drastic competitive injury to the smaller competitors might be a demonstration of anti-competitive effect sufficient to satisfy the statutory requisites even if the acquisition was truly conglomerate [8, page 10].

In the antitrust dictionary, "powerful" has no necessary connection with monopoly power or market control or even market share. It means "vast financial reserves," "overwhelming economic strength," "colossal corporate resources," or some other pompous polysyllabic combination meaning one four-letter word: size. And "drastic competitive injury" means simply loss of business. So a merger is illegal if (1) it involves a big company and (2) small business concerns afterward lose sales. Competition—pure, workable, effective, or whatever—has vanished, replaced by protectionism. Some small companies might have been losing business because they were inefficient or ill-adapted to new market conditions. Moreover, where firms are numerous, many must by chance alone be in difficulties as of any one moment. To blame their troubles, real or fancied, on the acquisition by the large company is clearly fallacious, *post hoc ergo propter hoc*.

The fallacy is, of course, logically distinct from the protectionist policy; either could exist without the other. Yet they are also parts of an antitrust mystique, according to which nothing ever happens by chance or because of economies of scale or general business conditions but always because of the big companies, unless this can be disproved beyond any reasonable or even unreasonable doubt. One does not really understand the antitrust process unless he has thumbed through pages

on pages of testimony by businessmen, "a-weepin' and a-wailin' how they done him wrong," and then seen how seriously this "evidence" is regarded. This is why many cases last too long—this and a staff burdened with too many cases and too little economics and statistics. But that is another story.

In conclusion: The very existence of a separate legal standard for mergers is surely debatable and it was assumed here not proved. But Mason has suggested that, although "in Sherman cases, the existence of going concerns argues for the application of—at best—a relaxed standard of market power, there is every reason why, in Section 7 cases, the standard should be strict" [10, page 40]. It is going to be strict when market power is involved, and it may be even stricter against economies of scale and integration. The enforcement of the Antimerger Act shows the same three-sided conflict—among competition, protectionism, and laissez faire or business statesmanship—which has prevailed throughout the history of the antitrust laws.

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POLICY IMPLICATIONS OF THE THEORY OF INTERFIRM ORGANIZATION

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Introduction

Public policy towards competition, it is said, is a maze of inconsistencies. The goals of the Sherman Act are thwarted not only by the "mean rapacity" and "monopolizing spirit of merchants and manufacturers,"[1] but also by other acts and government policies which foster monopoly and restraints of trade. Rarely do economists seem to be satisfied with the operation of the market economy. Studies unearth diverse elements of monopoly power which traditional theory suggests should be eliminated. Also evident are markets in which, save for government interference or immunities from the antitrust laws, competition would function better and markets with "sick" industries which conventional theory has difficulty in explaining.

The aim of this paper is to examine rather cursorily certain aspects of competitive policy in the light of the hypothesis that some form of interfirm organization inevitably appears in every market. Some of the alleged policy inconsistencies seem less pronounced and some sources of monopoly power appear less objectionable when viewed in this way. In addition, a few revisions in policy are suggested by the analysis.

A Summary of the Theory[2]

The theory of interfirm organization is based on the assumption that competition, as it appears in the real world, takes the form of interdependent rivalry. It is customary to assume that as the number of firms increases interdependence tends to disappear and that rivalry approaches the structural and performance characteristics of pure competition or the large-number case of monopolistic competition. It is assumed here that a more complex, linked form of interdependence emerges as the number of firms increases.

The complex form of large-number interdependence is itself divisible into two types. In the first—illustrated perhaps by retail gasoline stations—each firm can identify individually a few close rivals. Among the close rivals, oligopoly exists both subjectively and objectively.

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[1] Adam Smith, *Wealth of Nations* (Modern Library ed.), p. 460.

[2] For a more complete explanation, especially of the analogies between this theory and theories in the behavioral sciences, see my, "A Theory of Interfirm Organization," *Q.J.E.*, Nov., 1960.

Other firms comprise an amorphous group whose individual members appear to the first firm to have little direct competitive effects because of locational, service, and product differences. In the second type—and lumbering may be an illustration—individual sellers bargain bilaterally with buyers without identifying particular rivals. Rivalry is not subjectively oligopolistic, since individual firms do not recognize the effects of their own behavior on any of the others. Yet objectively, if not subjectively, interdependence remains.[3]

To avoid confusion with the traditional meaning of competition, the term "rivalry" will be used to denote behavior on the part of one firm which, because of positive cross-demand conditions, reduces the demand or profits of other firms. Rivalry need not be restricted to price rivalry, though that is undoubtedly the most important form.

The theory of interfirm organization is broader than conventional value theory in two respects. First, it is a theory of the behavior of the market group, not of the individual firm in the group. Second, the theory covers a complex of structural-behavioral-performance characteristics which conventional theory has compartmentalized into separate, sometimes competing, theories. Depending on the subject of inquiry, the greater generality may or may not be advantageous. In either case, the theory is complementary to, not a substitute for, the usual supply and demand analysis.

The amount of rivalry among firms is a function of many variables. Five of these are of particular importance: the number of firms in the group, the distribution of leadership power among them, the homogeneity of their value systems, the power of firms external to the group, or of other groups, to breakdown coalitions among the firms, and the formality of the interfirm organization.

According to the theory, independent rivalry tends to increase as the number of firms in the market group increases. This is a conventional notion which requires no elaboration. The nature of leadership, however, is less clear. Both its definition and measurement have eluded economists and behavioral scientists; yet its influence on group behavior is not denied. The power to lead and to prevent internal group conflict comes from many sources. Among firms, differences in size and the partial integration of some firms, making them suppliers or buyers as well as rivals, are perhaps the most important sources. Whatever

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[3] It ought to be noted, however, that in both types of complex, large-number interdependence some of the firms become aware of the market situation and attempt to form an interfirm organization to rationalize the behavior of all firms. The task of organizing is more difficult in the second type, since the fact of interdependence is less obvious to the individual firms being organized. The trial records of cases such as American Column and Lumber and Appalachian Coals are quite interesting when read in this light.

the source of leadership power, as its distribution among firms becomes more skewed the intensity of rivalry tends to diminish.

The homogeneity of value systems is nearly as elusive a variable as power. The value systems are similar if the firms have substantially identical production and cost functions, if their products are close substitutes, and if their managers have coinciding views concerning the equitable division of the market, the attractiveness of their group to entry by outsiders, the fair price, etc. And as the value systems of firms in the group become more alike, the advantage of and the desire for rival behavior tends to lessen.

The influence of what are usually regarded as complementary firms or groups is in the opposite direction. As the power of buyers or sellers increases, independent rivalry among the firms bought from or sold to tends to increase. Large buyers or suppliers—or effective coalitions of smaller ones—may force individual rather than group behavior on the part of the firms with which they deal.

Finally, the degree of independent rivalry tends to vary inversely with the formality of the interfirm organization. The organization may be quite informal, consisting of nothing more than the tacit recognition on the part of each that it is a member of a group which comprises all firms with which it has significant positive cross-demand relations. Just this recognition, however, may be adequate to mitigate rivalry, especially if the group is small and experience has demonstrated the consequences of independent behavior. From the point of view of the group, the primary function of the organization is to rationalize behavior by subordinating the efforts of individual firms to achieve their own subgoals. Without subordination, rivalry tends to make the entire group worse off. The organization works to the mutual advantage of all if to the unique advantage of none. In general, as the formality of the interfirm organization increases—that is, as more rules of market behavior are promulgated, accepted and enforced—the more the firms approach integrated management. The greater the formality, the more group behavior tends to replace independent rivalry.

An Examination of Selected Policies Towards Competition

Policies to Moderate Competition. Public policy has allowed or forced reductions in rivalry in many markets. Labor unions, agricultural and fishing co-operatives, and several other types of marketing agreements are specifically exempt from the antitrust laws. Fair trade laws and prorationing statutes limit the applicability of the antitrust laws. For a time, at least, the Robinson-Patman Act was used to reduce competition. State and local governments sanction many restric-

tions in the form of price controls, licensing and market allocations.

Almost without exception, the markets which have been accorded protection from rivalry are characterized by large numbers of sellers. In many—especially labor markets—the power of buyers to force independent behavior is great. In some—coal, petroleum, and milk production, for example—considerable heterogeneity exists in the value systems of the firms.[4] It is orthodox to regard these characteristics favorably and the exemptions given because of them unfavorably. Large numbers of firms, no aggregations of market power, and an absence of agreements restraining trade are commonly thought to produce desirable market performance.

The implications of the theory of interfirm organization are somewhat different. These conditions do encourage a high degree of rivalry but, because of its interdependent nature, an equilibrium similar to that of pure competition does not emerge. Rivalry extends over a broader performance spectrum than the poles of pure competition and monopoly. No rivalry is the equivalent of monopoly; a complete lack of interfirm organization, however, denotes such extreme rivalry and such an absence of behavioral norms that the market is chaotic. Equilibrium among interdependent firms—whether or not their rivalry is subjectively oligopolistic—requires an organization of sufficient formality to prevent independent behavior from destabilizing the market. At the extreme, that is, markets with a large number of equipowerful firms and very informal organizations tend to have such excessive rivalry that exemption statutes or some form of government regulation may improve performance.

It is difficult, though obviously necessary, to be more specific about the meaning of excess rivalry. Looked at negatively rivalry is not excessive nor even adequate in markets in which the interfirm organization is so formal that large numbers of smaller than optimum scale firms are perpetuated in the long run with positive profits. Similarly, even with optimum firms, rivalry is not adequate if prices fail in the long run to respond to changes in costs or demand. On the contrary, there are performance indicia of excess rivalry if prices are highly responsive to short-run changes in demand and if, because of this responsiveness and unrestricted entry into the market, even optimum-scale firms are forced from business. Chronic excess capacity, with new firms replacing those that fail, secularly low profits or persistent losses and a lack of technological change coming from within the industries are hallmarks of excess rivalry. Historically, if not theoretically, in-

[4] In each of these industries small firms which are hardly more than self-employed laborers working for little more than subsistence rival large corporate enterprises. Costs as well as subjective values differ greatly among the firms.

dustries with these performance characteristics do not exhibit self-correcting tendencies until conditions arise which, from an antitrust standpoint, mean less, not more, rivalry. The establishment of workable competition occurs mainly through some combination of a reduced number of firms, the rise of leadership power, and more formal interfirm organization.

Rivalry increased sharply in many markets in this country in the late nineteenth century. The effective number of firms in and the geographical size of market groups were increasing because of improvements in transportation and communication and the increase in population density. In most markets, excess rivalry was prevented or overcome by increasing the formality of interfirm organizations—agreements, pools, trade associations, etc.—or by forced or voluntary reductions in the number of firms and the rise of leadership—failures, mergers, and consolidations. But in some markets such changes did not occur. Labor markets, for example, had characteristics much like some of the exempted industries. Mergers were impossible because of the nature of the service being sold. Interfirm organizations—labor unions in this case—could attempt formality but could not achieve the requisite group behavior because of the power of buyers (employers) and the common law's and, later, the Sherman Act's prohibition of conspiracies. It was only after legislative exemption from the antitrust laws and, finally, the granting of an absolute right to bargain collectively that the private organizations became efficient in most labor markets.

As with labor, a formal interfirm organization may be necessary in some product markets if rivalry is to be restricted to the point that market performance is adequate to give minimum rewards to efficient firms. The extreme freedom of entry, the scattered resources or customers which determine the location of facilities, and the frequent lack of internal economies or presence of internal diseconomies—all work against the growth in the scale of plant and firm and the reduction in the number of firms which would be necessary for a less formal interfirm organization to operate efficiently.

On the other hand, even while a policy of moderating competition may be consistent with a total policy directed towards good market performance, absolute exemption from the antitrust laws and government aid in establishing formal organizations have in some instances gone too far. Excess rivalry has been replaced by too little rivalry. The retention of small, inefficient production units has been encouraged and prices have failed to respond to long-run market conditions. The answer is neither to ban nor to sanction particular organizations abso-

lutely but rather to allow the degree of formality which, while permissive of group equilibrium, is yet not so formal that the public interest suffers.

Policy under the Sherman Act. Exemption statutes treat specific industries, all of which tend to have similar characteristics. Their inflexibility is, therefore, partially justifiable. The Sherman Act is applicable to markets with diverse structural-behavioral-performance characteristics. Inflexibility in it may be of more consequence.

Section 2 of the Act is applied with a rule of reason which allows market analysis and flexible interpretation. In fact, the intent, power, and behavior criteria involved in the application of the rule can be translated into the basic variables which determine rivalry. Power, as used in Section 2 enforcement, can be analyzed in terms of the number of firms, the distribution of leadership power among them, and the countervailing power of buyers and sellers. Behavior concerns the formality and the functioning of the interfirm organization. Intent and behavior relate to the value systems of the firms.

Section 1 is inconsistent with Section 2 in this respect. Fairly specific legal treatment has developed for each of several ways in which the market organization may restrain rivalry, the treatment in each instance being predicated primarily on the form of the restraint, not on its effects when considered in the context of the other variables which mutually determine the degree of rivalry. Trade associations, for example, may legally perform certain functions and may not perform others. The dichotomy between lawful and unlawful functions is apparently the same whether there are five or five hundred firms in the group. And price-fixing agreements—agreements which in any way affect prices or tamper with price structures—are per se illegal even if they improve market performance.

It is curious that Section 1 has been interpreted with so little regard for effects. Several of the more important precedent cases involved factual situations not radically different from those which have resulted in exemptions from the antitrust laws. The Traffic Association cases, the Addyston case, Maple Flooring, Trenton Potteries, Appalachian Coals, and Socony-Vacuum—all presented depressed conditions and excess rivalry prior to the agreements which led to antitrust action. To an appreciable extent, the motives underlying the private agreements were the same as those which led to immunity statutes.

If it is admitted that rivalry is a function of several variables, that because of its oligopolistic nature rivalry may be excessive as well as too restrained, and that even with optimal firms an interfirm organization of some degree of formality must exist to rationalize independent

behavior, the inflexible approach which has developed under Section 1 cannot be justified. Every interfirm organization affects prices and price structures if it is doing even the minimal job of preventing market chaos. Section 1 enforcement may, indeed, have succeeded in preventing many unreasonable restraints of trade. Its emphasis on form rather than effects, however, may also have prevented the same type of restraints from being used in the context of other market circumstances in which the restraints would not have been unreasonable with performance criteria. To the extent that this is so, enforcement has encouraged the development of alternative ways to reduce rivalry. Some of the very industries which provide precedent cases for Section 2 policy—railroads, coal, petroleum—have been granted immunity from the antitrust laws. Others demonstrate the alternative of reducing the number of firms through mergers and failures.

It is impossible to judge whether immunity statutes would never have been passed had Section 1 been interpreted with a rule of reason. Similarly, one cannot estimate which mergers would not have occurred. Probably on balance the section has prevented more unreasonable restraints on rivalry than it has caused. A rule of reason approach, however, would have permitted the prevention of the same unreasonable restraints and, at the same time, would have discouraged the passing of exemption statutes and the use of mergers as alternatives to more formal organization.

Conscious Parallelism. The emphasis on form rather than effects in Section 1 enforcement has caused particular problems in treating consciously parallel behavior. With but a few firms, rivalry may be inadequate for good market performance even though the interfirm organization is completely informal and tacit. The few firms, keenly aware of their membership in a small group and of the consequences of independent rivalry, develop through experience a common code of behavior. In simple oligopoly, the small number of firms is an effective substitute for formal organization.

There can scarce be doubt that, in a behavioral sense, agreement exists among the few firms. Each is reasonably certain of the others' patterns of behavior; their value systems are much alike. Each acts in contemplation—frequently justified—that the others will react in a particular way. In terms of market effects, these tacit agreements restrain as much rivalry among a few firms as more formal agreements do if firms are more numerous. But, since agreements which affect prices are illegal, recognition of these tacit agreements by the courts would make simple oligopoly illegal per se. To avoid this result and to maintain legal consistency between express agreements cases and conscious

parallelism cases, a fictional dichotomy of hypotheses is erected. One hypothesis is agreement. The other hypotheses concern facts which might reasonably explain the parallel conduct without agreement. If none of the latter is found, the agreement hypothesis is accepted and the per se rule applied.

The degree of rivalry is actually a blend of individually motivated and group motivated behavior. The absence of group behavior, especially in matters of price, means excess rivalry in oligopolistic markets. The absence of individual behavior is the equivalent of monopoly. The agreement hypothesis stresses group behavior to the neglect of individual behavior; the alternative hypotheses stress individual behavior to the exclusion of group. An analysis of the market would show the presence of both and lead to an evaluation of the effects of their mixture on market performance.

Recent decisions suggest that market analyses are made in fact in conscious parallelism cases. The courts tend to find agreement only when the parallelism is complex, predatory, or otherwise unreasonable.[5] Group behavior which has good market effects is ignored and only that which is unreasonable is labeled agreement. Conscious parallelism cases can be brought into consistency with Section 2 cases by giving explicit recognition to the implicit test of reasonableness which is currently conducted. And if the test were explicit, there would be greater likelihood of its being well conducted.

The Policy Implications

The economic objective of competitive policy in a market economy is to promote good performance—good in the sense that resources are used efficiently (if not ideally), that progress through technological change is encouraged, and that economic activity contributes to, or at least does not interfere with, the achievement of equally important social and political goals. Market performance depends on the degree of rivalry among firms. If it were true that performance improves monotonically with increases in rivalry and that rivalry itself varies monotonically with a set of structural and behavioral variables, there would be a place for inflexible, per se rules in competitive policy. Under these circumstances, efforts to maximize (or minimize, if that is the nature of the function) any one or all of the independent variables in the function determining rivalry would improve performance.

The theory of interfirm organization suggests the abandoning of

* See Bernard Sorkin, "Conscious Parallelism," *Antitrust Bul.*, 1957, Richard A. Givens, "Parallel Business Conduct Under the Sherman Act," *Antitrust Bul.*, 1960, and Almarin Phillips and George R. Hall, "The Salk Vaccine Case: Parallelism, Conspiracy and Other Hypotheses," *Virginia Law Rev.*, May, 1960.

per se rules, not because of prejudice in favor of the use of discretion rather than rules, but because of the more complicated and more general relationship it describes among structure, behavior, and performance. The immunity statutes appear less inconsistent with achieving desirable market performance when analyzed with this theory than with traditional value theory only because of the view that performance is not monotonically related to rivalry. To a point, increases in rivalry improve performance. The point, however, is not uniquely defined by any one of the variables which determine rivalry. Per se rules which establish either absolute legality or absolute illegality depending on a condition of but one of the variables may improve or may worsen market performance.

Actually, the change in policy which the theory suggests is not great. One suggestion is the elimination of complete immunities from the Sherman Act—a change which the Supreme Court has now read into at least one immunity statute[6]—and the substitution of a rule of reason. Without the latter, the elimination of per se legalities appears quite impossible. The immunity of labor unions, for example, could hardly be abrogated if the alternative were to subject them to the per se illegality doctrine currently used for Section 1 enforcement.[7]

The other suggestion—really but the reverse of the first—is that the per se illegality doctrine be dropped. As noted above, the economic if not the legal aspects of the doctrine appear to be abandoned currently in parallelism cases which require inferential proof of agreement. The substitute rule of reason should foster enforcement in circumstances which produce too little rivalry even though no express agreement can be found since the crucial test would be performance, not the fact of agreement. The same rule would, of course, allow some formal agreements now per se illegal, but this would occur only when other variables tend to create a high degree of rivalry and when alternatives to formal organization—mergers and exemptions—pose as serious threats to market performance as the formal agreements. Thus the implications of the theory of interfirm organization do not constitute an apology for monopoly. When compared with current policies, the suggestions favor more competition in every case save those in which extreme rivalry is present.

The problems of enforcing a rule of reason cannot be minimized. Perhaps these negate the value of the argument even if its foundation

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[6] *Maryland and Virginia Milk Producers Ass'n.* v. *U. S.*, 168 F. Supp. 880.

[7] For a more complete discussion of the monopolistic aspects of union activities and the applicability of the Sherman Act, see James W. McKie, "Collective Bargaining and the Maintenance of Market Competition," in *The Public Stake in Union Power*, Philip D. Bradley, ed. (Charlottesville, 1959).

is logically valid. The difference between enforcing per se rules and the rule of reason can easily be exaggerated, however. The rule of reason does not ordinarily require exhaustive investigation of the market. The requirement is only for adequate detail to draw inferences concerning the performance of the particular market.[8] When the facts are complicated, a prolonged trial may indeed result, but this is also the case when a per se rule is applied. It may take as long to determine whether the complicated facts fit the category of behavior the per se rule proscribes as it does to assess their impact on performance.[9]

[8] Compare Edward S. Mason, "Market Power and Business Conduct: Some Comments on the Report of the Attorney General's Committee on Antitrust Policy," *A.E.A. Papers and Proceedings*, May, 1956, reprinted in *Economic Concentration and the Monopoly Problem* (Cambridge, 1957), especially p. 398.

[9] See James A. Rahl, "Per Se Rules and Boycotts Under the Sherman Act," *Virginia Law Rev.*, Nov., 1950, p. 1173. It may be recalled, too, that the trial in Socony-Vacuum was conducted with use of the per se rule; yet the record ran to more than 12,000 pages in addition to more than 1,000 exhibits.

MERGERS AND CARTELS: SOME RESERVATIONS ABOUT POLICY

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I. *The Beginnings of Doubt*

People who give their lives to promoting radical changes in the economic order are not usually destined to enjoy a happy old age. Most probably they will look back on a lifetime of failure. If their proposals find favor, they are especially likely to suffer the disenchantment that comes with success. The reformer who fights the good fight for wild life conservation, a policy of bills only for the Treasury, or international student exchange has more modest hopes and hence is less vulnerable to disappointments. But the man who would nationalize General Motors—or, alternatively, break it up into five smaller units—is likely to feel in his hour of victory that he has labored in vain. The Pareto distribution of income, the organizational chain of command, the balance-of-payments problem, and the human propensity to truck, barter, and exchange still endure. Or he may even feel that he has prophesied falsely; that the owners, managers, workers, and customers of General Motors had better have been left to arrange the production of cars through the chaos of an imperfect market. Given the evangelical atmosphere in which antitrust problems have been discussed in this country, I do not think the Biblical allusions too farfetched.

When I first learned about antitrust problems twenty years ago, it was a nearly self-evident proposition that "antitrust has never been tried." And since there was no immediate prospect that it would be tried, one could plot revolution in the coffee house without danger to oneself or the American economy. In the last two decades, antitrust has finally been tried with a vengeance. It will almost certainly be tried on an even bigger scale in the next two decades. How the antitrust laws come to supply a sizable fraction of all cases heard in federal courts is an interesting subject in itself. I will pass it over at this time to confess that, as a fellow traveler of the antitrust agencies of long standing, my faith, in their hour of success (victory is too strong a term), is not what it once was. I hasten to add that much of my faith remains; and that, if it finally ebbs away entirely, I expect to be left an agnostic and not a convert to a rival theology—some variety of state socialism or a rationalization of the *status quo*.

Most of the time in economics—and the same is probably true of

other subjects—we are content with a plausible explanation of a puzzling phenomenon. If A follows B and we can think of a good reason why A should follow B, we pass on to another topic. I do not believe that this haste makes us reprehensibly unscientific. One can argue that only people with simple notions of causation ever achieve much; that the exacting men who strive for the whole truth never manage to arrange their results in a way that is intelligible to anyone else.

But, of course, simple notions of causation that may help sophomores in a principles course feel at home in the world are not necessarily an acceptable basis for economic policy. Since judges, as well as sophomores, now have need of a theory of mergers and cartels, we might profitably reconsider our simple treatment of this subject. I submit that it runs about as follows: Most markets are not perfectly competitive. When competition is already imperfect, mergers and cartels will make it more so. Competition should not become more imperfect. Therefore, mergers and cartels should be discouraged.

No economist—not even a veteran of the Antitrust Division—would subscribe to this view of mergers and cartels without appending a long list of qualifications. Yet, we seldom bother to make the qualifications. Most of the time we write and act "as if" we accepted the validity of the simple and direct approach. Admittedly, a combination of previously independent firms may create market power which will be used to restrict output. The welfare cost of this restriction imposed by the redistribution of income may exceed the benefits of rationalization. But then again it may not. It is not self-evident that a policy which discourages mergers and cartels does mainly good.

II. *The Case for Combination*

In the case of cartels we might note that, the more elaborate the restrictive covenant, the more likely it is to make a contribution to greater technical efficiency. Yet it is the elaborate cartel agreement that is most vulnerable to a hostile public policy. In fact, American law has virtually eliminated the cartel which superintends the engineering and marketing decisions of its member firms. In legal theory virtually all cartels—unless explicitly sanctioned—are criminal conspiracies. In practice, gentlemen's agreements are so ubiquitous that they cannot be suppressed; and state governments seldom bother to prosecute even open conspiracy under their little "Sherman acts." Hence, the main casualty in the antitrust war has been the ambitious cartel which requires an extensive organization to control its members' operations. Likewise the loose coalition of business rivals that has no object except the restriction of output is the type least affected by federal and state harassment. Of the other major merit sometimes conceded to cartels it is not

necessary to speak at length.[1] The objection that it is unfair to prevent
an industry with many producers from doing through a formal cartel
what a highly concentrated industry can do with a tacit understanding
has often been made. To my knowledge the objection has never been
persuasively rebutted.

Cartel policy in the United States has at least been consistent for
sixty years. While the same cannot be said for our treatment of merg-
ers, the drift of the law in the direction of greater federal control has
been fairly steady. It has now reached the point where no industrial
firm numbered among the two hundred largest will be allowed to buy a
major competitor. Indeed, any firm in this select company will probably
provoke an antitrust suit if it tries to buy a major supplier or customer.
The slow development of merger policy can be taken as evidence of the
innate good sense of Congress and the courts. For as mergers have
more pervasive effects upon production and marketing arrangements
than cartels, so their costs and benefits are more difficult to weigh.

Most mergers, of course, have virtually nothing to do with either the
creation of market power or the realization of scale economies. They
are merely a civilized alternative to bankruptcy or the voluntary liqui-
dation that transfers assets from falling to rising firms. "Civilized" is
perhaps an inadequate word. As a method of transferring capital from
one management to another, the merger is superior to bankruptcy or
voluntary liquidation since it avoids the loss that inheres in the destruc-
tion of any going concern.

In this connection, we might digress to note the relation that inter-
nal growth may be expected to bear to external growth in an industry.
If the capital market were perfect and a merger conferred no monopoly
power, a rising firm would be indifferent between the two forms of ex-
pansion. The ratio of assets transferred by merger (i.e., external
growth) to the industry's net investment (total growth) would be a
function of this net investment and the average length of life of the
firms comprising the industry. The more rapid the industry's expansion
and the shorter the life cycle of its firms, the greater the part that
mergers will play in the expansion of successful firms. Hence, it is not
surprising, that southern textiles—a rapidly expanding industry with a
rapid turnover of firms—affords so many examples of impressive ex-
ternal growth. So that mergers most commonly indicate not the decline
of competition but its undoubted vigor.

It remains for us to explore the connection between market imper-
fection and mergers that confer economies of scale. The tie is not

[1] For a thoughtful case against the categorical banning of cartels in American law, see
Almarin Phillips, "A Critique of United States Experience with Price Fixing Agreements
and the *Per Se* Rule," *J. of Ind. Econ.*, Oct., 1959.

obvious. When production is technically inefficient in the sense that "eventually" unit cost can be cut by concentrating production in a smaller number of larger plants, the guidance of the Invisible Hand will presumably bring about the desired result. Unit revenue will fall below unit cost for existing size plants as some firms re-equip with larger and more efficient plants. Existing size plants will not be replaced as they wear out; i.e., when total receipts fall below total variable costs. And the industry will ultimately achieve a new no-profit-no-loss equilibrium with a population of fewer and larger firms. Mergers will not hasten this adjustment in an imperfect market—indeed they will hinder it. Mergers can only make an imperfect market more imperfect. By conferring monopoly power that encourages a restriction of output, mergers reduce the rate of plant obsolescence and so retard the introduction of improved equipment.

Where then are the economies of scale that can only be realized through consolidation? The following catalogue is not exhaustive; but I believe that it includes most of the scale economies that are likely to require the attention of the antitrust agencies in their task of distinguishing good and bad mergers.

1. Mergers increase efficiency to the extent that they reduce "standoff" sales efforts; that is, outlays on sales promotion that a firm undertakes only because it dare not abandon them unless its rivals follow suit. Given the volume of advertising in the modern world, this waste is prima facie substantial; and, insofar as the vigilance of the antitrust agencies prevents oligopoly from more closely approximating monopoly via mergers, one cost of decentralization is perpetuated.

2. Mergers can increase efficiency when they involve firms which are overtly or tacitly sharing the market without, however, distributing output quotas in a way that makes the marginal cost of production equal in all firms. Since in the United States firms are forbidden to negotiate elaborate cartel agreements, it is a safe assumption that some amount of technical inefficiency inheres in any *sub rosa* market sharing agreement.

3. The gale of competition may cease to blow before the industry has grouped itself into firms of optimum size, so that mergers remain the only way of doing the job. So long as the number of firms in a decreasing cost industry is fairly large, the replacement of existing plants with larger plants will encourage the exit of some firms. But once the process of elimination has concentrated production in a handful of firms, it may cease. For now, though victory in a fight to the finish may promise an assured market position to somebody, the remaining survivors may collectively feel that the gamble is not worth taking. There is an additional consideration here. The fewer the firms in the

industry, the easier it is to avoid the misunderstandings that lead to mutually unprofitable price wars and the combinations that serve to eliminate them.

4. Finally, rationalization via an elimination contest may be out of the question because contending firms possess advantages which cannot be duplicated by rivals or exploited in the most profitable manner without the co-operation of rivals. A classic example of this situation is the preposterous patent stand-off where two firms possess patents which, in the absence of combination or cross-licensing, they cannot exploit without infringing each other's grant.

If there exist scale economies that can only be achieved by merger, it follows that any policy that reduces the incidence of mergers has a cost in the sacrifice of technical efficiency. While the magnitude of this cost is difficult to estimate, I do not think that we ought to dismiss it as insignificant. This opinion is based upon the visible wastes of oligopoly in such highly concentrated industries as soap, aluminium, steel, automobiles, and tobacco—industries noted for large advertising budgets, bogus product differentiation, cross-hauling, excessive service, etc. My impression is that there is nothing inevitable about these wastes— that they would be among the first casualties of the wave of mergers that would follow a repeal of the antitrust laws. Some time ago I contended that the whole theory of oligopoly was a backhanded tribute to the effectiveness of the antitrust policy.[2] I reasoned that in its absence businessmen would have no cause to prefer the guessing game of tacit collusion to a straightforward conference. For direct negotiation is the efficient way of arriving at a treaty of mutual assistance or at least a nonaggression pact. This reflection on the effectiveness of antitrust was meant as a compliment. Now I am not sure that it should be so interpreted.

III. *Merger Policy and Concentration*

In any event, the United States in the next few years, through nobody's act of conscious planning, will try a most important experiment. The vast majority of mergers have nothing to do with monopoly; and the work of Professor Adelman indicates that concentration ratios in the industrial sector of the economy have not perceptibly increased— if they have increased at all—in the last thirty years.[3] But we also know with reasonable certainty that widespread merger activity has helped to perpetuate high concentration ratios. Or that it served this

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[2] *Monopoly in Economics and Law* (Chicago, 1959), pp. 303-04.
[3] "The Measurement of Industrial Concentration," *Rev. of Econ. and Statis.*, 1951. Given the declining importance of mergers in the growth of large firms, Adelman's skepticism about the oft-alleged "trend to monoply" is as justified now as it was ten years ago.

end until about 1945 when the affluence of the antitrust agencies began to make growth by merger difficult for the two hundred or so largest industrial firms. Assuming that this hostility in the law persists and intensifies, we shall soon ban growth by merger in very large firms while allowing them to expand by constructing their own facilities.

If technological progress is a centrifugal force in the American economy, then concentration ratios in most industries should decline. If Khrushchev, Galbraith, and Lilienthal are correct in supposing that research and development exalt the large firm, then the ban on mergers will not keep the four or five largest firms in most industries from maintaining or increasing their share of total output. Prophecy is an imprudent but accepted pastime in our discipline. So projecting present trends into the future, I would guess that the results of our experiment in banning mergers while allowing internal growth will confirm the preconceptions of economists who are biased in favor of smallness. That is, since mergers have not increased concentration in the last thirty years, their curtailment should reduce concentration in the next thirty years. You will forgive me if I decline to guess about the magnitude of the anticipated fall in concentration. And as noted above, I do not doubt that some major industries will behave perversely because they really do have as yet unrealized scale economies.

In this connection we might note that the case for cartels is tied very closely to the case for mergers. For the greater the tenacity of high concentration ratios when mergers are banned, the stronger the presumption that cartels also can contribute to the least-cost combination of manpower and materials.

IV. *Two Suggested Revisions of Antitrust Policy*

You will see that my misgivings about merger and cartel policy do not add up to a clear-cut set of recommendations for changing the antitrust rules. My suggestions, in fact, rest as much upon administrative expediency as on economic principle. Since mergers and cartels have both good and bad results—often simultaneously—an ideal policy would perforce consider each disputed coalition "on its merits." But, as we know, faithful adherence to any rule of reason entails legal complications too fearful to contemplate. Some inconsistency is inescapable. My own particular variety takes the form of urging federal neutrality toward cartels and hostility toward mergers.

If collusion cannot be suppressed—and if there is no unimpeachable reason why it should be—we ought to discard the rule that makes collusion criminal conspiracy per se. Yet if the law should not oppose the negotiation of cartel agreements, neither should it enforce them. This proposal rests on the premise that the social utility of cartels is still an

open question and, hence, that public policy should not be obliged to take a stand.

As regards mergers, my preference is that the present experiment in discouraging growth by merger should be continued (at least for some trial period); and that the ban on mergers should be extended to all industries which are not, by common consent, good examples of workable competition. The reasons for believing that economies of scale are not enough to promote further concentration in most industries have been set down above. If this prognosis is wrong, no great harm will be done so long as firms remain free to expand by building their own facilities. In decreasing cost industries, the necessity of relying upon internal growth will ordinarily serve only to slow up the process of concentration. High-cost small firms will not be abolished by merger; but they will still disappear through bankruptcy and voluntary withdrawal. The only enduring damage inflicted by a tough merger policy will be in those decreasing cost industries already so highly concentrated that a fight to the finish is mutually abhored for its obvious dangers.

My proposals relating to our treatment of mergers and cartels contemplate only minor revisions of antitrust policy and they are submitted with no sense of urgency. My main plea is rather that we recognize that antitrust has its costs; that these costs may be more than nominal; and that economic theory is an unsure foundation on which to base the case for antitrust. If our yardsticks of merit are technical efficiency and the rate of increase in man-hour productivity, the case for our present treatment of mergers and cartels is not that it does much good; but rather that, on the basis of available evidence, it does not seem to do much harm. So that if the policy promises to make a contribution to other desired ends (notably, the preservation of family businesses, organizational diversity, the elimination of unsavory trade practices, and fair play in bargaining), it ought to be pursued.

V. *The Modest Promise of Antitrust*

In the end, we are driven back to a truth so trite and simple that we seldom voice it in discussions of antitrust for fear of seeming naïve. The truth is that now, as in Adam Smith's age, the first foundation of competition is freedom of contract and not restraints on freedom of contract designed to discourage its use for bad, i.e., monopolistic, ends. One authority has chided the common-law judges of the nineteenth century for their inability to distinguish between freedom of contract and freedom of competition. I submit that while judicial logic may often have been at fault, judicial instinct was sound enough.[4]

[4] W. L. Letwin, "The English Common Law Covering Monopolies," *Univ. of Chicago Law Rev.*, Spring, 1954, pp. 381-85.

If competition is a hardy plant, it requires little beyond freedom of contract for its salvation. If competition is a fragile plant constantly endangered by unfriendly technical advances, lunch hour conspiracies, and the success of big firms in scaring potential rivals away from profitable preserves, not much can be done for it by law. Or more accurately, it is unlikely that a society which places as high a value on technical prowess as our own, would consent to pay a very high price to block mergers and cartels.

But to say that the law can do little for competition is not so say that it can do nothing. In agriculture and the garment trade, the law of mergers and cartels is superfluous. In the marketing of shoe machinery, the law is probably pernicious; for here even scale economies relating only to the plant are so conspicuous that the perpetuation of the existing duopoly (United Shoe Machinery and Compo) seems wasteful.[5] Yet there is surely an area between nearly pure competition and "natural" monopoly where a policy that discourages combination can be both effective and beneficial. This area includes, at the very least, such important industries as electrical appliances, petroleum products, the building trades, processed food, motor transport, and drugs.

So long as one believes in the existence of this sector of the economy, the faith remains that antitrust policy promises to do a better job of promoting efficiency and progress than unfettered freedom of contract. Many of us feel that the desire to improve economic performance has played a minor role in the shaping of antitrust policy—that Congress and the courts have usually been more concerned with improving business ethics, preserving small firms, and discouraging coalitions which, by ganging up on rivals, customers or suppliers, offend our notions of fair play. We are loath to accept, however, that our professional influence in this world is so slight that criteria of efficiency and progress have no place in antitrust law.

[5] In 1953 the court understandably refused to order the dissolution of United Shoe Machinery Corporation—a long-time target of the Antitrust Division—because its production had become concentrated in a single plant. *United States* v. *United Shoe Machinery Corp.*, 110 F. Supp. 295, 348 (D. Mass., 1953).

DISCUSSION

JAMES W. McKIE: All of these papers call for modification of current anti-trust policy. Economists are entitled to offer such advice and criticism whenever they can contribute insights, distinctions, definitions, or generalizations based on scientific economic principles. Policy recommendations involve value judgments in addition, but economic policy requires economic analysis as the first step. Professor Adelman's paper, for example, is almost entirely concerned with correct definition of "monopoly" and "market power," and with consistent distinctions among the various kinds of integration that mergers produce. Without such definitions and distinctions, policy operates in an atmosphere of chaos, as he points out. The conflict between competition and protectionism in antitrust policy is not going to be resolved by correct definition, of course, but without it we cannot locate the source of the trouble. As for Professor Adelman's observations on policy, I certainly concur in his judgment that the standard of enforcement of Section 7 against horizontal mergers should be strict. Perhaps the difficulty with vertical mergers could be lessened, if not eliminated, by permitting the merging firms to offer a positive justification that the merger would enable them to secure economies of integration by a means more efficient than internal expansion. Probably no one thinks that all vertical acquisitions are innocuous, even though their effects may be less direct than horizontal ones and though they do not create the monopoly that they may transmit or enhance.

The papers by Professors Phillips and Dewey offer more revolutionary doctrines along with their policy recommendations. The Phillips paper draws its policy implications concerning price agreements and similar arrangements partly from the theoretical problem of oligopoly price equilibrium and partly from the problem of "excessively competitive" or "sick" industries. If I may oversimplify, its main position seems to be that internal group conflict (and hence chaos) in these industries is prevented by "interfirm organization" and that the antitrust law should recognize the worthiness of this end and validate this means of achieving it. This implies abandonment of the law on cartels and the per se doctrine of conspiracy under Section 1 of the Sherman Act. Professor Phillips recognizes that price and output in an oligopolistic market may differ quite widely from the results of pure monopoly, depending on the looseness of its structure, and restates this as an attribute of "interfirm organization." The boundary at the other end of the spectrum between excessively competitive and ordinarily competitive industries is unclear; Professor Phillips appears not to recognize the latter as a valid case. Instead, oligopoly is ubiquitous, and large numbers merely make oligopoly more chaotic.

One cannot ask for thorough empirical study, nor even a very detailed theoretical exposition, within the scope of a panel discussion. Without dwelling on the need for better support of the general thesis of this paper, therefore, I shall merely mention what appears to me as the outstanding weakness of

its policy recommendations: the lack of any statement of workable perform-
ance standards to support the proposed "rule of reason" on price-fixing agree-
ments. This is not surprising, considering the well-known difficulties surround-
ing formulation of such standards. But I wish to emphasize that economic
science has not been able to construct reliable chains of influence between
such performance desiderata as equity, stability, or progress, and various forms
of market organization, to say nothing of the possible conflicts between these
norms. The only reasonably scientific statements we have been able to make
about desirable performance relate to allocative efficiency, which is linked to
pure competition. Without much fuller evidence I am not prepared to believe
that competitive markets, imperfect as the competition may be, are usually
"sick" and in need of the therapy of price fixing, nor to accept the substitu-
tion of other criteria of performance until we have been told how to judge
performance. It seems to me that the motives of businessmen who desire to
escape the discipline of competition (or perhaps to transfer their sickness to
the consumer) can easily be understood in terms of the ancient explanation
offered by Adam Smith. It is interesting to speculate on what defenses the
electrical equipment manufacturers would have offered in the recent Philadel-
phia price-fixing cases under the rule of reason proposed in this paper, and how
much more convenient they would have found it to use a theory of interfirm
organization rather than a code based on the phases of the moon.

My reaction to a similar proposal in Professor Dewey's paper is similar.
In truth I am unable to discover why he recommends a hostile policy toward
mergers and a neutral policy toward cartels; the first part of the paper seemed
to support a neutral or even benign policy toward both. In any event, Pro-
fessor Dewey's view of the function and purpose of cartels is different from
mine, and there is no opportunity here to evaluate the evidence. I do not be-
lieve that any known cartel has actually made any appreciable "contribution
to greater technical efficiency" nor, certainly, has any such efficiency been re-
flected in cartel prices. The dominant motive in cartel formation is equity:
the protection of vested interests and "fair" division of a limited market, com-
bined with a kind of quietism that regards competitive stress of any kind as
the ultimate evil. Luckily cartels often break down; otherwise most would
evenuate in perfectly static and almost minimally efficient monopoly. But
again I am merely restating traditional doctrine; and perhaps I have been
looking at different cartels from those Professor Dewey has been examining.
The traditional premise is that the social utility of cartels is a closed question,
and it is not likely to be reopened until some impressive new evidence is on
hand.

REUBEN E. SLESINGER: Professor Adelman's able presentation points to the
dilemmas confronted in the application of the antimerger features of the anti-
trust laws. Precedents are vague, as most complaints are of recent origin and
most have been settled through consent procedures. The philosophy behind
the antitrust laws is not always clear, but seemingly believes that the com-
petitive process is the ideal—that it prevents control. Professor Adelman points
out clearly that other results, such as assuring a more effective utilization of

resources or benefiting consumers, have not been the major legislative intent of the laws but rather an unexpected significant consequence. The fundamental thesis is that most of our antitrust laws have been enacted in the process of protecting some sector of the business community.

It would be possible to mention numerous modifications of the antitrust laws, both of a restricting and relaxing nature, that have been passed as the result of pressures to protect some type of business, with less concern over market effects. To cite but two, we have the Robinson-Patman quantity discount law and the numerous retail price maintenance statutes. These fit aptly into Adelman's judgment that power to exclude others (usually the less efficient) is the keynote behind not only antimerger enforcement but most of the antitrust laws as well.

The antimerger provisions of the Clayton Act give the Justice Department and the courts a powerful tool because of their vagueness. These provisions are written in general terms, allowing a broad area for interpretation. Thus the courts are concerned with incipient monopoly and probabilities as well as with actual results. Competition is not defined; as Adelman says, it is "rather a hypothesis confirmed by the available evidence." One may question the relative wisdom of the courts in their interpretations of what constitutes competition and what tends to affect it.

One of he areas in which this vagueness leads to trouble is the definition of the market. One might be tempted to believe that the courts prejudge the issue, decide what they wish to hold, and then define the relevant market accordingly. In one Du Pont case, for example, the relevant market was broadened to include all sorts of flexible wrapping materials; but in the second case, the idea of the relevant market was narrowed to cover only special types of finishes and fabrics. In a recent district court trial, a merger between Continental Can and Hazel Atlas Glass was upheld. The concept of the relevant market was viewed as each type of container, and so the merger added nothing to market control by the acquiring company. If this decision is upheld, there might be considerable question raised as to the ability of the government to proceed against circular and conglomerate mergers.

Professor Adelman reviews ably the situation with reference to horizontal, vertical, and conglomerate mergers. The issue is rather clear for horizontal mergers. The matter becomes one of degree, however. Whereas Hudson and Nash merged to form American Motors, a similar merger between General Motors and American Motors would be banned. Where is the stopping point? Criteria aside from size are suggested, such as nature of the product, substitution, and demand and supply.

The issue is not so clear for vertical mergers. There may be many important advantages to both the acquired and acquiring companies. The complaint usually is that the merger permits extension of market control into adjacent areas, particularly affecting buyer-seller relationships. Vertical integration becomes an offense because it interferes with the protection of some business—old customers, supply sources, and the like. Another vague aspect of the problem is that of quantitative substantiality in the attempt to quantify the degree of interference involved.

In the case of conglomerate mergers, the parameters of permissive action are still less clear. A departure from the basic idea of protection is suggested in judging these cases since the profits from each activity are not linked with each other. There is some possibility, however, of looking at the newly formed firm as being so large after the acquisition that its size alone does exert an influence, and so there might be some question even here of protection against size. One must agree with Professor Adelman that dual standards in the enforcement of antitrust laws are undesirable. But the task is not easy in trying to reconcile the objectives of competition, protection, and laissez faire.

Professor Dewey presents some of his concerns over the basic objectives of antitrust policy that are centered around improving business ethics, preserving small firms, and discouraging coalitions that result from ganging up on rivals, customers, or suppliers. This is in line with the theme of protection as a core of the antitrust laws. He questions, however, the minor role given to improving technical efficiency and progress. The record of antitrust proceedings causes some question over this point. Perhaps the discrepancy results from a variance between the philosophy of the legislators responsible for the statutes and the pragmatic interpretations handed down by the courts. Many of the recent decisions in cases arising out of the antimerger provisions, as well as a number of the older trade association issues, might be justified more under the first set of criteria than under the promotion of technical efficiency.

The attempt to provide a theory for mergers and cartels that might be applied in both legislative and judicial interpretation does not resolve the problem, since, as Dewey points out, a long list of qualifications is required. Further, one might raise the question of the desirability of having economists frame this theory, since there is great difference among them as to what is desirable public policy: unfettered competition, regulated industry, or government ownership. Once a statute is enacted, questions arise again whether jurists should interpret specific cases at issue according to the advice of economists. Seemingly, it would be admissible for them to seek the advice of economists to analyze possible consequences of action or to interpret specific market behavior; but the prime mission of the court is to enforce the law as written, not as economists might prefer that it be.

The simple approach is indicated as one that assumes that competition is desired, that most markets are not purely competitive, and that mergers and cartels add imperfections, and so they should be discouraged so as not to produce any additional imperfections. Qualifications develop at this point. Although a combination may result in the appearance of market power, it is possible that rationalization of industry may bring greater social welfare benefits than the offsetting hindrance to competition that existed before. The possibility of "yes" and "no" indicates the difficulty faced in constructing an all-embracing antitrust philosophy. Many cartels enhance technical efficiency, and yet it is true that this type of combination comes under the greatest public censure. The antitrust laws give no clear-cut guidance for resolving this issue; they are vague and general. But, perhaps, this is on purpose, since under juristic interpretation the courts will be in position to consider each

issue on a case-by-case basis, relying on precedent only where desired to out-line the framework. The Appalachian Coal case may be cited as illustrative of the fact that in interpreting specific issues, the court may look at the prac-tical situation and permit what otherwise might be entirely forbidden, if the practice in the specific instance is deemed necessary for the welfare, efficiency, or regularization of the industry.

It is important to remember also that a considerable degree of imperfection exists in markets where it is difficult to prove overt cartel or merger action, as Professor Dewey tells us, "gentlemen's agreements are so ubiquitous that they cannot be suppressed. . . ." Loose coalitions of business rivals are quite com-mon and many restrict output (something the courts interpret as a per se violation of the antitrust laws) as effectively as an overt pool. The whole theory of oligopolistic action, especially with mutual recognition, is called into ques-tion. One might ask how does this lead to "conscious parallelism" and when does it become suspect. The matter of degree must be considered again as a possible explanation. What might be accepted as rational oligopolistic action among smaller firms probably would be thrown out as an antitrust violation among larger concerns. Thus "it has now reached the point where no indus-trial firm numbered among the two hundred largest will be allowed to buy a major competitor."

In developing a public policy, Professor Dewey would modify the law with reference to cartels since some of these might be good and some might be bad, but he would retain the antimerger laws pretty much as at present especially for those areas of industry that are not covered adequately by concepts of workable competition.

No truer statement could be made than the comment of Professor Phillips that "public policy towards competition . . . is a maze of inconsistencies." The apparent conflicts between traditional theory and market practice are indicated, economists being unsatisfied with market phenomena in many instances, recognizing that competition might function more effectively in some cases, yet being aware of the fact that there are "sick" industries. One of the anomalies of market behavior is the fact that some leading industries such as bituminous coal and agriculture that approach the attributes of pure competition are termed sick, and attempts to alleviate their distress either through governmental action, co-operatives, or industry self-help revolve around agreements and regulations that limit the degree of individual market competition.

Professor Phillips argues that some type of interfirm organization appears in every market and that real market competition takes the form of interde-pendent rivalry. Further, as the number of firms increases, interdependence becomes more complex and linked, instead of moving closer to pure competi-tion as premised in theory. If I may add at this place, it points to the necessity for distinguishing between degrees and kinds of competition. Economic analysis has been accustomed to look upon competition as being identified with the pure type, at least assuming that this is the equilibrium ideal. A vacuum then is created for the many cases of oligopoly and monopolistic

competition. May we suggest that competition be used as a generic term meaning rivalry or interdependence, and that there then be set up various classes of competition—pure, oligopolistic, product differentiation, and the like; but that the degree of rivalry between the firms is not related to the kind of competition. There might be greater rivalry, for example, between a few oligopolistic steel producers than between many coal operators.

An interesting list of factors is presented that interact to determine the amount of rivalry between firms. A point to be remembered is that interfirm organization shifts emphasis from firm behavior to the group and that the greater the degree of formal organization, the more the group behavior tends to replace independent rivalry.

Public policy has not followed a consistent philosophy even under the antitrust laws, exempting a wide range of activities or weakening the effect of the statutes in particular instances.

A unique characteristic of interfirm organization is the fact that its absence tends to produce not a state of desired pure competition but often a completely chaotic type of market behavior. Another paradox is that the industries with excess rivalry where chronic excess capacity exists, profits are low, and losses are persistent and technological progress is limited generally do not have a record of self-correction, and so types of interfirm organization are adopted which the antitrust laws do not approve. (The Appalachian Coal case may be cited as an example here.) Phillips suggests that the exemption statutes and interpretations would have been less necessary had there been more of a rule of reason used in connection with Section 1 of the Sherman Act relating to combinations and conspiracies. Complex problems arise also in squaring the legal concept of conscious parallelism with the mutual recognition of oligopolists—a type of interfirm behavior.

Good market performance is set forth as the goal of competitive policy in a market economy. Such behavior is hard to identify but it would promote effective resource utilization, technological progress, and the attainment of national goals. It is suggested that per se illegality of interfirm behavior be eliminated and that each case be judged on its own standards of market performance.

JEROME B. COHEN: To paraphrase Gilbert and Sullivan, here we have a paradox, a most ingenious paradox. At a time when antitrust enforcement has moved ahead vigorously and some telling blows have been struck, three distinguished antitrust economists express dissatisfaction with varying aspects of current antitrust policy.

Professor Adelman complains that enforcement policy appears to be aimed more at protecting competitors than competition. Professor Phillips maintains that enforcement policy emphasizes form rather than effects, that current enforcement via the inflexible per se rule has encouraged alternative ways to reduce rivalry, and urges the extension of the rule of reason to areas now dominated by per se doctrine. Professor Dewey indicates that his faith in antitrust is ebbing. He feels that it limits economic efficiency and restricts technological progress. He favors a more benevolent view of cartels and agrees

with Professor Phillips that the per se rule should be discarded. He urges that we recognize that antitrust has its costs.

These are thoughtful and provocative dissents. One can agree with some aspects singled out for emphasis. Certainly the Bethlehem-Youngstown case provided the most nightmarish definition of a market in almost all of judicial history. And the now famous "nibble"theory enunciated in the Brown Shoe case makes a mockery of any reasonable view of what "may tend substantially to lessen competition" means.

Professor Phillips notes that price-fixing agreements which in any way affect prices are illegal per se even if they improve market performance. I can think of one case, currently being contested, where a foreign car importer, attempting to break into the United States car market and finding, in due course, such wide product acceptance that it could not for years meet the demand, faced a per se price-fixing charge because it attempted to persuade dealers to hold down the car's price to the suggested level and not gouge purchasers. Clearly, this improved market performance, increased market penetration, and enhanced competition. Yet the company has had the per se rule thrown in its face.

Such excesses and such rigidities are painful but do they warrant the gamut of pessimism and distress which these papers manifest? I think not.

A recent survey by the National Industrial Conference Board, analyzing fifty-seven cases in which corporate mergers or acquisitions have been challenged by the government under Section 7 of the Clayton Act, over the past ten years, found that effect on the market rather than the size of the companies involved appeared to be the major consideration for Clayton Act anti-merger action on the part of the government. Of the forty-three merger suits instituted against manufacturing companies in the period studied, more than one-third were directed against companies that ranked below the three hundred largest manufacturers. Actions were taken against only 2 per cent of all the recorded merger or acquisition moves made by the three hundred largest manufacturing companies. A not unreasonable record, is it?

Take price fixing. Was not per se illegality pretty much warranted in the apparently open and shut conspiracy of the electrical equipment manufacturers? On the other hand, in the cases of the oil companies and of the Salk vaccine producers, the government failed to convince the courts, beyond a reasonable doubt, that conspiracies to fix prices existed.

Or, back to mergers again, against Brown Shoe you can place Continental Can. And in Foremost Dairies, the FTC examiner ruled that nine of the acquisitions should be divested while forty others should be allowed to stand. Perhaps it is this varied response of the tribunals to enforcement actions that trouble some. But then the force of criticism should not be directed either to enforcement policy or to decision making but rather to the imprecision and vaguity of the statutes. What is a market? What is a line of commerce? What is a section of the country? May an action tend substantially to lessen competition? These are argumentative words, indeterminate words, obscure words. Poor words for a statute, you may argue. But what are the alternatives? Would you go along with those who argue that to meet the Soviet threat we

need to abandon the doctrine of competitiveness in domestic industry and turn to regulated giant cartels, paralleling in size and power the Soviet industrial trusts? Or would you reword the law to read that in the case of any industry when a company secures a 20 or 25 per cent share of the market, measured say by net sales, it should be regulated as a public utility, have its rate of return prescribed (à la A.T.&T.), and if and when it reaches a 40 or 50 per cent share of the market, it should be ordered to divest a given percentage of its business?

Are we prepared to go in these directions? Or is it better to put up with uncertainty, an occasional inept decision, varying enforcement policy, an imprecise statute, enjoying the intellectual stimulation of the moves and countermoves in which lawyers, economists, and judges indulge in most antitrust cases? Personally, I enjoy chess.

ECONOMIC ANALYSIS OF URBAN PROBLEMS

INTRA-URBAN LOCATION PROBLEMS: AN EVALUATION*

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The "Frontiers of Economic Knowledge" is the central theme around which papers of this meeting are organized. The topic of this paper, intra-urban location problems, ventures beyond the frontier and off into the wilderness. This is because the topic is too involved, practically limitless, and relatively unexplored.

This paper is concerned with one aspect of intra-urban location problems: the potential of intra-urban location theory to the solution of urban problems. No attempt is made to develop a theory of intra-urban locations. Rather, what is sought is a current appraisal: What are the urban problems? What can we say about them? Finally, where do we go next? It is important to consider such questions as these, for regardless of the state of our theoretical models, planners are planning, developers are developing, and highway engineers are pouring ever more concrete.

After a few words pleading the case for urgent attention to intra-urban location problems, this paper considers: a pure-market, intra-urban, "location assignment" problem, the approach to a solution via agricultural location theory which, in turn, highlights the crucial role of governments in intra-urban locations and suggests, possibly, the most important void in the theory of the urban structure.

The Urgency of Attention

Without defining the term "problem," just about everyone agrees that urban areas face problems. Moreover, in the era of "the exploding metropolis" the magnitude of these problems is on the increase. It is of interest to note, for example, that two leading economists, Alvin Hansen and Albert G. Hart, not primarily specialists in urban problems, take the position that urban problems will be the most challenging economic problems in the next twenty-five years.[1]

Other evidence attests to the demand that something be done about urban areas. At the time of this writing, there is speculation that President-elect Kennedy will call for a new cabinet post on urban affairs.

Possibly this position is designed simply to co-ordinate the myriad of federal agencies already concerned with urban problems. On another front, the Committee for Economic Development has already issued a policy statement, "Guiding Metropolitan Growth," and further statements by CED are under consideration. Finally, experimentation has gone on in an effort to solve some urban problems as exemplified by the new governmental units created in Miami (Metropolitan Dade County) and in Toronto (Metropolitan Toronto). No doubt a good deal of additional evidence could be cited to illustrate concern about the urban region. What does all this and other evidence point to?

At the risk of being overdramatic, this writer suggests that, just as the depression of the thirties strained public faith in market forces, the failure to solve urban problems in the sixties may again tax public faith in market forces.

Not all of these problems, of course, involve space. Yet, many problems have a spatial context: "gray belts" surround central cores; slums blight whole areas; and congestion affects most urban dwellers. Can intra-urban location theory help us evaluate and solve some of these problems? For purposes of exposition, it is useful to set forth in a simplified form a conceptual intra-urban, location assignment problem which market forces seek to solve.

The Intra-urban Location Assignment Problem

The nature of a pure, intra-urban, location theory assignment problem may be conceptualized in the spirit of von Thünen and Lösch.[2]

Imagine a bounded nonagricultural region. A large population engaged in a set of productive activities is introduced. Included in these activities are dwelling units.

To keep things simple, eliminate all considerations of geographical terrain; i.e., activities are located on a smooth undifferentiated surface. Further, assume a transport surface exists. This implies that the transport costs of a good are constant for any given distance in any direction. (This says nothing about the level of transport costs; they may be high or low.) In general terms, the task is to find a general equilibrium solution which allocates these activities in urban space. The resulting patterns, leaving the term "patterns" and their measurement undefined, represent the spatial allocation of urban activities via pure market forces.

One approach to the assignment of economic activities in urban space has been to apply agricultural location analysis.[3] Consideration of agri-

[2] Johann H. von Thünen, *Der isolierte Staat in Beziehung auf Landwirtschaft und Nationalökonomie* (Hamburg: Fr. Derthes, 1826); August Lösch, *The Economics of Location*, trans. by William Wolgom with the assistance of Wolfgang Stolper (Yale Univ. Press, 1954). The term "assignment" is not used here in the tight technical sense, but as shorthand for the spatial allocation of activities.

[3] See William Alonzo, "A Theory of the Urban Land Market," *Regional Sci. Asso. Papers*

cultural theory not only provides some insight into urban patterns but, in addition, points up certain difficulties which suggest new areas for investigation.

Agricultural Location Analysis

Agricultural location theory has been relatively well explored.[4] Many of the explorations are variations on a theme by von Thünen. Von Thünen imagines a central city surrounded by farm lands. Some distance away from the city is an impenetrable circular forest bounding the farm lands. His task is that of assigning agricultural crops to various zones.

In essence, transport costs to market for the various products taken in conjunction with demands allocate land to various product belts. In this analysis, the belts are concentric rings around the central city. The technical details of the analysis are not relevant for purposes of this discussion. What is relevant are two of the assumptions common to discussions of agricultural location theory.

One assumption is that of a transport surface, just as Weber and Lösch assume a transport surface for their nonagricultural analysis. True, modifications of simple "cost proportional to distance and weight" analysis have been introduced, but these modifications do not vastly distort the patterns of location. How unrealistic is the assumption of a transport surface?

Let me suggest that for the United States it is not unrealistic. Between the railroads, highways, and waterways, we have a grid that approaches a transport surface. Whole areas are not inaccessible. In consequence, we do see broad belts; cities surrounded by truck garden crops; milk, cream, and butter zones; and other patterns, explicable in terms of transport-demand considerations.

A second assumption, standard in most discussions of market forces, is the absence of externalities; i.e., private costs and benefits are equal to social costs and benefits. In the agricultural sector, this seems to be a reasonable assumption. Robert Frost is our witness. In his "Mending Wall," Frost's neighbor argues that "good fences make good neighbors." His stand is adamant despite Frost's declaration that:

> He is all pine and I am apple orchard.
> My apple tree will never get across
> And eat the cones under his pines.

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and Proceedings, 1960, pp. 149-57; Walter Isard, Location and Space Economy (Technology Press, M.I.T., and Wiley, 1956), pp. 200-06. An excellent summary and extension of intra-urban analysis is given in Barclay G. Jones, "The Theory of the Urban Economy" (unpublished doctoral thesis, Univ. of North Carolina, 1960). Jones's review of the literature supports my contention that we have not yet developed general equilibrium of the urban economy. What we have are some suggested approaches.

[4] See Edgar S. Dunn, The Location of Agricultural Production (Univ. of Florida Press, 1954) and the references cited therein.

Less elegantly, Mr. Frost sees no externalities.

These assumptions plus others combined with market forces enable transport cost-demand variables to assign rents such that agricultural zones are created. A simple piece of casual evidence suggests that the theory and the assignment of agricultural locations is not bad. While we have heard many discussions of agricultural problems, the "problem of bad locations" does not arise.

How does this analysis apply in intra-urban space?

Intra-urban Location Patterns via Agricultural Location Theory

Agricultural location theory does suggest some spatial patterns in terms of the intra-urban location assignment problem specified above.

Given an urban core of some kind, we would expect to find located in that core activities where: (1) face-to-face requirements are high; (2) specialized human labor inputs are needed which may be available outside of the core's large labor pool; and (3) the market is the whole region; i.e., speciality, consumer oriented activities. Outside the core, the single storied manufacturing activities which cannot afford high land rents could be expected. Mingled in with these manufacturing activities are some residential units. And probably the furthest distance from the core would contain the "estate dwellers."

In terms of key variables, these patterns emerge as the result of: (1) technology; e.g., the technology of mass, assembly line production which requires a low single storied manufacturing plant; and (2) communications; e.g., the need for face-to-face contacts versus telephone communication. Transportation facilities and technology are a third key variable. Recall that for our assignment problem a transport surface has been assumed. To the extent that transport costs on the transport surface are low or high, patterns may be expected to expand or contract. Congruent with these patterns, of course, is a rent map of the urban area.

All of this suggests, so far, that the application of agricultural location theory is relevant for intra-urban regions. These patterns are pretty much those described by Hoover and Vernon for the New York metropolitan region.[5] Given the objective of the Hoover and Vernon analysis, which is evidently to take a broad look towards the future, an agricultural type of analysis seems satisfactory.

Yet it does not distract from this type of analysis to suggest that this is not enough. Policy-makers, whoever that group may be, seem to require more specific information; i.e., more detail as to locational patterns. Here an agricultural type of location analysis breaks down.

[5] Edgar M. Hoover and Raymond Vernon, *Anatomy of a Metropolis* (Harvard Univ. Press, 1959).

The difficulty in applying agricultural location theory to the intra-urban region may be simply stated: the assumptions simply do not fit reality. First, urban areas are not transport surfaces and, therefore, simple rings do not emerge. Second, a more critical assumption: the lack of externalities is not valid. Externalities are omnipresent.

The Absence of a Transport Surface. Urban regions, clearly, are not transport surfaces. Even Los Angeles is not an undifferentiated sea of concrete, in spite of the efforts of highway engineers. More often than not, major and minor arteries lead out from the core of cities.

As a result of arteries, urban regions become combinations of rings and arterial spokes; i.e., star shaped. To consider an urban region only in terms of rings can be very misleading. For example, consider the twenty-five to forty mile spoke out of mid-Manhattan running through Connecticut. This segment includes the commuter towns of Greenwich, Stamford, and Darien. If the segment is moved counterclockwise, a less densely populated region emerges, until the eastern shore of the Hudson River is approached. As even the reader unfamiliar with this area can guess, these denser strips lie along the commuting railroad lines.

This is not the place to ask if there is some optimal number of arteries. Whether a five, six, or *n* numbered star appears is not relevant. What is relevant is the simple realization that the patterns of urban development we see are not those that emerge from market forces operating on a transport surface. The patterns that result are dependent upon a transport network.

The transport network can well be included as an endogenous variable in any model relying on market forces; e.g., as Lösch has done. No doubt, many intra-urban bus routes are determined by market demands. Yet, the real world is full of imperfections; bridges take time to build; intra-urban railroads may temporarily provide "indequate" service in the absence of certain tax advantages; some cities may rely on rapid transit for the movement of people while others read the demand for mobility as a call for more freeways. In short, intra-urban location patterns reflect a transport network, and not a transport surface. Thus, agricultural patterns are modified. The transport network may well result from nonmarket as well as market forces. Hence, patterns are further distorted.

Perhaps the greatest difficulty in applying agricultural location analysis to the urban economy arises when externalities are considered.

Externalities in Intra-urban Space. The usual rules of the game allow only market forces in solving general equilibrium problems. Thus, in imagining any solution to the pure intra-urban location assignment problem stated above, no restrictive governmental actions are allowed.

This implies no zoning, control of air pollutions, and other such governmental restrictions and activities. Obviously, it is difficult to imagine what sort of locational patterns might emerge, given the large number of externalities.

The presence of a substantial number of externalities in the urban area arises, in part, in the nature of externalities. Externalities have a spatial extent. While Farmer Jones's chickens and pigs may smell up his barnyard, they do not bother Farmer Brown. Space can isolate or internalize externalities. Yet, as any resident of Chicago can testify, the wafts from the stockyards on ripe days are something less than invigorating. In the case of urban areas, they simply do not have the space to internalize externalities.

Of course, externalities may be positive. Whereas Farmer Jones's attractive front lawn may mean little to Brown, who rarely sees it, in an urban environment the whole neighborhood may enjoy one homeowner's striking landscape.

Geographically, externalities are of varying size. Some are confined only to a neighborhood; e.g., the attractive lawn. Others cover the whole region; e.g., air pollution.

Given the large number of externalities, suppose we seek a stable solution to the intra-urban location assignment problem without government restraints such as zoning. What would be the result? Without rigorous proof, it seems likely that an unending game of musical chairs would result. Maximizing its own advantage, a manufacturing firm may locate in the middle of a residential neighborhood. In turn, the residents may move only to find later another manufacturer in their midst. In short, I am suggesting that a problem exists with even more destabilizing elements than in the simple location assignment problem tackled by Koopmans and Beckmann.[6] Yet, their simple assignment problem had no stable solution.

The achievement of more stable solutions which recognize social costs and benefits may be accomplished when government action is introduced. The control of externalities in the urban region, which can only fall upon governments, removes some of the destabilizing elements. Unlike Frost's view on rural fences, we may analogize that zoning, for example, seeks to make good fences because good fences do make good urban neighbors.

Urban patterns then, to a much greater degree than agricultural patterns, are molded by governmental decisions. The form and mode of transport and the policies to control externalities are all important. In

[6] Tjalling Koopmans and Martin Beckmann, "Assignment Problems and the Location of Economic Activities," *Econometrica*, Jan., 1957, pp. 53-76.

turn, the elements of urban problems are more clearly seen if we reconsider intra-urban location patterns as a joint product of government plus market decisions.

The Intra-urban Location Problem Reconsidered

The twofold source of urban problems is analogous to the sources of error in national income forecasting models. Two sources of error are possible: (1) the endogenous relations are less than perfect; e.g., the consumption function predicts poorly; and (2) exogenous variables are set incorrectly; e.g., government expenditures are higher than assumed.

Suppose we want to solve some urban problem, say traffic congestion. A "good" solution requires: (1) an ability to predict market reactions to an exogenous change; e.g., shopping behavior after the introduction of new one-way streets; and (2) that from the set of possible actions the government chooses the proper action; e.g., new one-way streets versus a new parking lot with shuttle bus service. Errors from either the first or the second imply a degree of nonoptimality.

Stated positively, good solutions require an ability to predict patterns which result from market forces, given governmental actions. Here new theories as well as the extensions of agricultural location theory can help to build better predictive tools. Good solutions simultaneously require appropriate governmental actions. It is the latter requirement that is much neglected and really involves a theory of the spatial aspects of urban finance. Examples will indicate the nature of the problems involved in evaluating the government component.

Consider an urban renewal program. Suppose slums are replaced by low-income housing. Assume part of this program is paid for by taxes on the rich. In turn, the rich regroup in a suburban tax colony. As a result of both actions, urban patterns are altered. The slum has been removed, and, hopefully, the neighborhood reclaimed. The rich, now further out in the urban landscape, require commuting facilities. Other ramifications can be imagined.

The question is: Have we a good solution to an urban problem? Clearly, this action has resulted in a redistribution of income. Hence, the goodness or badness of the solution must, at least, consider local governments' function in income redistribution.

Recently, there has been an increased call for area-wide planning as a solution to urban problems. This is a subtle issue.

Area-wide planning is necessarily ineffective in the absence of area-wide government for the functions involved. With area-wide government, such solutions are good only to the extent that, through the political process, the government has somehow ascertained voters' views

on the externalities involved. Without ascertaining preferences, we cannot be sure that governmental actions are solving urban problems consistent with consumer-voter sovereignty.

Summary

In summation, the solution to problems involving the organization of intra-urban space (1) requires a recognition of the role of the government in controlling externalities and influencing the transport network and (2) given governmental restraints, a theory which can predict location patterns. Both are important.

Yet this study, which originated in an attempt to evaluate the possible contribution of intra-urban location theory to the solution of urban problems, leads to the concluding judgment that, in terms of ordering research priorities, further studies on the role of the government win hands down.

CONTRASTS IN AGGLOMERATION: NEW YORK AND PITTSBURGH

By Benjamin Chinitz
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The natural inclination of a scientist when confronted with a new problem is to try to solve it with old tools. When he is finally convinced that the old tools will not do the job, he retreats to his shop to fashion some new tools. The burden of my argument in this paper is that we have reached the stage in regional economics when we must begin to fashion some new analytical tools.

When I say regional economics I have in mind specifically the analysis of the growth and structure of the economy of geographic subdivisions within a national economy. This type of analysis is now being carried on in at least a dozen metropolitan areas throughout the country and in numerous other types of subdivisions, large and small. I have been associated with two such studies: the New York Metropolitan Region Study, which just recently published its final report, and the Pittsburgh Economic Study, which is at its halfway mark, having been initiated in June, 1959, and being scheduled for completion in June, 1962. My observations, as the title of my paper suggests, are drawn from these two immersions in regional economics.

The basic-nonbasic approach to the analysis of a region has been under severe attack from many quarters in recent years. But I think it is fair to say that alternative approaches have differed in degree of refinement more than in kind. Fundamentally, we still go about our business in the same way. We try to identify the autonomous influences operating on the region and chart a network of interdependence between sectors within the region. I have no quarrel with this approach. I find it difficult to frame the problem otherwise. My quarrel is with the limitations of the maps of interdependence which are typically drawn.

I will surely be doing some of my colleagues an injustice with the following generalization but, begging their pardon, I state it anyway: our efforts so far have been almost exclusively devoted to the demand dimension of interdependence. The supply side has been virtually ignored. Let me elaborate.

The basic-nonbasic model is a way of coming to grips with the demand side of interdependence in one fell swoop. The links in the output-income-consumption chain, the links in the output-capital-

formation chain, the links in the output-tax revenues-government spending chain, and the links in the output-materials purchased-output (i.e., input-output) chain, are all subsumed under one grand link between the exogenous and the endogenous elements of the system. Sometimes we can get away with this leap over a lot of treacherous ground just as in football a seventy-yard pass from the thirty-yard line occasionally results in a touchdown. To maintain the metaphor, most of us prefer to gain more yardage on the ground before passing into the end zone.

So we move in small steps. We try to chart the flows between our sectors in greater detail. A dollar of output of industry A, we observe, generates a demand for the output of industry C which is not equal to the demand generated for the output of industry C by a dollar of output of industry B. We observe, further, that a dollar of output of industry A generates more or less personal income than a dollar of output of industry B. If we are really keen observers, we might even discover that a dollar paid out to workers in industry A generates demands for consumer goods, housing construction, and government services which are different from the demands generated by a dollar paid out to workers in industry B.

My point is that in the main we improve upon the crude basic-nonbasic approach by a process of flow disaggregation—a process which hopefully will reduce our margin of error. I characterize this activity as the application of old tools to new problems for the obvious reason that input-output relationships, consumption functions, investment functions, and the like are old tools which were fashioned to solve the problems of a national economy. Furthermore, all those tools are used to come to grips with the demand side of the interdependence between sectors in a regional economy.

When I say that the supply side has been ignored, I mean simply that we have not come to grips with the following question: How does the level of activity in industry A in a given region influence the factor supply curves confronting industry B in the same region? Let me hasten to exclude one kind of effort from my indictment. We certainly have tried to incorporate the influence on industry B of the availability of the output of industry A as an input to industry B. Probably the best example of this kind of work is the Isard-Kuenne study of the impact of the Fairless Works. But this is only one of a number of supply relationships which need to be explored and, as far as I can tell, they have not received adequate attention from regional economists.

My former colleagues on the New York Metropolitan Region Study staff could certainly register a strong objection at this point. After all, another term for supply interdependence is external economies and

diseconomies, and there is certainly a lot of discussion about them in a number of volumes of the New York study.

But this discussion is limited to two problems: one has to do with intraindustry external economies and diseconomies the other has to do with the influence of the aggregate size of the region on the costs of individual firms. The first problem is too narrowly defined and the second too broadly defined from my point of view.

Nevertheless, I believe, regional economics owes a great debt to the New York Metropolitan Region Study for highlighting these external relationships. It was only after we were confronted with the problem of understanding certain features of the Pittsburgh economy that we at the Pittsburgh Study felt compelled to probe more deeply into the nature of these interindustry effects.

Pittsburgh, as a metropolitan economy, stands in sharp contrast to New York with respect to these three summary variables: size, industrial structure, and rate of growth in recent decades. New York is between six and seven times the size of Pittsburgh. New York has a much more diversified industrial structure. And, while New York has grown at just a bit less than the national rate for the last thirty years, Pittsburgh has grown at less than half the national rate in the same period.

Superficially, all these contrasts fit a familiar pattern. Large areas are more diversified than small areas. Diversified areas exhibit more stability in their growth because their fortunes are not tied to the fortunes of a few industries. In these terms, Pittsburgh's story seems easy to tell.

Unfortunately the matter cannot rest there. Pittsburgh is much more specialized than any large SMA with the exception of Detroit, including many which are no larger than Pittsburgh and many which are considerably smaller. The question, why is it not diversified, therefore, remains largely unanswered. Of course, if we could accept the lack of diversification as inevitable, we might not have to try to understand it. For it is difficult to quarrel with the proposition that the future of such an area can be safely projected once we project the future for its one or two dominant industries. But here we may be caught on the horns of a dilemma. Suppose we project a sharp decline in the dominant industries along with a modest decline in the region's minor industries. True, the dominant industries will retard the growth of the region but in the process they will also decline in relative importance. The region will then become more diversified in its old age, so to speak. What then? Do we correct for the increased diversification? Does it open up new opportunities to the region?

The need to understand the whys and wherefores of diversification should therefore be quite apparent. This has led us to consider the

question which I posed earlier: How does the growth of one industry in an area affect the area's suitability as a location for other industries?

But we are not yet ready to assert that the latter question has to be answered. We might avoid it if we could show that different degrees of diversification in areas of comparable size are due simply to the fact that some areas have a variety of locational advantages which makes them attractive to a variety of industries while other areas offer advantages only to a small number of industries. Observe for example the figures in Table 1 for Cleveland and Pittsburgh.

TABLE 1

EMPLOYMENT IN SELECTED MANUFACTURING INDUSTRIES, 1957

	Cleveland	Pittsburgh
Food	14,532	20,459
Textiles and apparel	14,130	3,550
Printing and publishing	14,618	10,042
Chemicals and products	17,959	6,823
Stone, clay and glass	3,260	21,372
Primary metals	46,894	154,215
Fabricated metals	38,378	31,298
Machinery, nonelectrical	52,552	23,534
Electrical machinery	20,746	27,652
Transportation equipment	55,570	11,047
Total	311,471	358,239

SOURCE: Bureau of the Census, *Annual Survey of Manufactures*, 1957.

Pittsburgh is way ahead in glass and primary metals and leads also in food and electrical machinery. Cleveland, on the other hand, is ahead in textiles, printing, chemicals, fabricated metals, nonelectrical machinery, and transporation equipment. On the whole, Cleveland is a much more diversified manufacturing center. But maybe this is just the outcome of the process by which individual industries gravitate to those areas which are best for them. If Cleveland had attracted the 154,215 employees in primary metals, it might still look more like itself than like Pittsburgh in the rest of its manufacturing profile.

I cannot assert positively that this is an unsatisfactory way of approaching the matter, but I can suggest a number of reasons why I find it necessary to go beyond it. For one thing, this approach implies that location of industry is heavily determined by transportation factors or, as we say in our jargon, transport oriented. By this we mean that the location of markets and materials and the transport network determine the geographic distribution of industries. If a lot of industries end up in one place, presto, you have a diversified regional economy.

Nobody believes that the logic of location runs in these terms for the majority of manufacturing industries. My former colleague, Robert

Lichtenberg, of the New York Study, after a painstaking review of factors influencing industrial location classified 50 per cent of American manufacturing as nontransport oriented. P. Sargent Florence has repeatedly emphasized in his writings that transport orientation is a minor influence in location. There is also a fairly general consensus that the proportion of industry which is transport oriented is diminishing as time goes on.

Once we recognize that variations in production costs are important determinants of location, we cannot avoid the consideration of interindustry factor cost relationships. Production costs are not given by nature, except that nature may influence the cost of energy and the cost of plant. These are trivial determinants alongside the influence exerted by the way in which a region's natural advantages are exploited. If we ask why are wage rates higher in one area than another, it is only in rare cases that nature will provide the answer. In most cases the explanation will run in historical terms; that is, in terms of the heritage of each region as it bears on labor supply.

For many purposes it is sufficient to recognize the difference in wage rates, and there is no compulsion to explain why it exists. A firm which is seeking a maximum profit location for a new plant might very well take the wage-rate differential as given—a type of behavior which fits so well the textbook model of a competitive firm. Even so, a conscientious consultant might very well post a warning signal. After all, a plant represents a twenty-year commitment. What is the wage differential likely to be twenty years hence? Be that as it may, it is certainly inappropriate to take wage-rate differentials as given in a twenty-year projection for a region. A static atomistic approach will not do for a problem in aggregate dynamics.

I also find the multiple-locational-advantages theory of diversification unsatisfactory for another reason. It permits us to assess an area's potential for growth only with reference to industries with known locational requirements. But in a projection, it is difficult enough to anticipate the bill of goods, let alone to project the locational needs of the industries which will be producing them. This may suggest that we ought to give up the ghost. Those who have this alternative are blessed. The rest of us have to seek ways to mitigate the curse. One is to develop the concept of a region's capacity for attracting new industries with considerable freedom of location from a transport point of view. If we are to develop such a concept, we need to probe into the region itself more deeply than we do when we locate industries one by one.

I have said enough—perhaps too much—about my reasons for raising these questions. I will now proceed to the main business of this paper, which is to offer some hypotheses about interindustry influences

on factor costs. To begin with, I think that the net has to be spread a lot wider than most people assume. I propose to consider all the traditional categories: entrepreneurship, capital, labor, and land, in that order.

Entrepreneurship. This is a production factor which, to my knowledge, no one has tried to price out at different locations. The implicit assumption, I suppose, is that the supply schedule of entrepreneurship is identical at all locations. Our colleagues studying international differences in growth reject this assumption explicitly. I am convinced that we need to reconsider its validity in regional economics.

When you tell a location analyst that a firm is where it is because its founders prefer to live there, he throws up his hands. Such cases, he claims, are outside his domain. Our own experience suggests that for many industries cost is almost invariant with location—or at least there is no "min min" location. Yet we are reluctant to treat such cases as random phenomena because we feel there are significant variations in the cost of entrepreneurship. Moreover, these differences may be large enough to offset other cost differences.

I came to this notion by reflecting on the differences between New York and Pittsburgh, but I hasten to say that area size is only one variable. For a given size of area, the entrepreneurial supply curve is also a function of certain traditions and elements of the social structure which are heavily influenced by the character of the area's historic specializations.

The proposition I offer is this: An industry which is competitively organized—in the neoclassical sense of the term "competition"—has more entrepreneurs per dollar of output than an industry which is organized along oligopolistic lines. The average establishment in the apparel industry, for example, has one-sixth as many employees as the average establishment in primary metals. Furthermore, multi-unit firms account for 82 per cent of the employment in primary metals, while they account for only 28 per cent of employment in apparel. Now you may have as much management per dollar of output in primary metals as you have in apparel, but you certainly do not have as many managers who are also risk-takers and this is my definition of an entrepreneur.

What is the consequence of this? My feeling is that you do not breed as many entrepreneurs per capita in families allied with steel as you do in families allied with apparel, using these two industries for illustrative purposes only. The son of a salaried executive is less likely to be sensitive to opportunities wholly unrelated to his father's field than the son of an independent entrepreneur. True, the entrepreneur's son is more likely to think of taking over his father's business. My guess

is, however, that the tradition of risk-bearing is, on the whole, a more potent influence in broadening one's perspective.

I think I have formulated a proposition which can at least theoretically be tested, although I confess that I have not tested it yet. For all I know, this may already be a well-established proposition in entrepreneurial history.

But if an oligopolistic environment has a lower entrepreneurial birth rate, there remains the question of how receptive it is to the in-migration of entrepreneurs. Here, too, I would argue that the competitively organized area has an edge. Receptivity as measured by factor costs we shall discuss under separate headings later on. What I have in mind now is receptivity as it relates to the entrepreneur's "utility function." There is an aura of second-class citizenship attached to the small businessman in an environment dominated by big business. It manifests itself in many small ways, such as the kinds of social clubs he can belong to, the residential areas he will comfortably fit into, the business organizations he can join, and so forth. The ease of entry, to borrow a concept from industrial organization, is considerably greater in an environment dominated—not dominated, to be more exact—by small firm industries. I am not sure that we can satisfactorily test this notion, but I am hopeful.

Capital. Many of the same observations are relevant to regional differences in the availability of capital. Here, too, we are dealing with a factor whose cost is typically assumed to be invariant with respect to location. This is surely not so. It is true that capital is almost perfectly mobile, provided the probability distribution of returns is approximately known. G.M. and U. S. Steel can raise capital almost anywhere with equal ease. But a small firm embarking on a new enterprise will find a much more receptive ear over the home counter than it will over-the-counter in "foreign" places. The cost of transferring confidence may be high enough to give us a capital-supply function which has distance as an important independent variable.

Once we admit of such immobility, it becomes relevant to inquire into differences in local capital supply among areas. Again the industrial organization of the dominant industries strikes me as an important variable. A major source of capital to new firms in general is the undistributed profit and unexpended depreciation allowance of old firms. Now, the surplus capital which accumulates inside large multiplant companies, I would argue, is more mobile interregionally within the company than intraregionally outside the company. A large corporation is more likely to respond to investment opportunities in its traditional activity at other locations than to investment opportunities at home in

unrelated industries. The small firm, by contrast, is more likely to make its surplus capital available to other local enterprise in another industry than to a distant enterprise in the same industry. (Actually, I have overstated the case to avoid a complex formulation. All I need to argue is that the marginal rate of substitution between local and foreign outlets is greater [smaller] for the large multiplant firm [small firm]. Given an equivalent array of investment opportunities at home, the surplus capital of the multiplant industry is more likely to "leak" out to other areas.)

The commercial banks, of course, also play a vital role in the initial financing of new business. Are banks in one area more receptive than banks in another area to the demands of new business and, if so, are these differences in attitude shaped by the industrial traditions of the area? I say yes, on both counts. My conviction on this point is based less on deductive than on inductive reasoning. I have been told that this is the case. Having been told, I can think of some fairly good reasons why this might be the case. When banks cater to a competitively organized industry, they are more likely to accept the insurance principle of making money, not on each customer, but on the average customer. If you have U. S. Steel and Westinghouse on your rolls, you do not have to learn to make money on the insurance principle.

In the present state of my knowledge, I am not too optimistic about being able to test these hypotheses empirically. However, I am not prepared to pronounce them as untestable. This is an altogether too easy way out. I believe if we think hard enough, we can spell out some corollaries which, if we dig hard enough, we can subject to empirical test.

Labor. Now we come to what most will assume and I am prepared to concede is the cost factor, which is most sensitive to interindustry influence. Yet, even here, I suspect I will be spreading my net farther than most people would.

First, the wage level. My colleague on the Pittsburgh Study, Mel Bers, is exploring this question in great detail. The presumption that the wage level in the dominant industry influences the wage level in other industries is one which no one can seriously question. I am confident that Bers's research will throw more light on the network of interdependence than anything that has been done so far. Bers is also immersed in the study of the influence of labor organization in the dominant industry on the structure of wages in the region. These two issues are inseparable in his framework.

But there are other influences relating to labor cost and supply which are not generally recognized. Bers found, for example, that the rate of participation of married women in the labor force in the

Pittsburgh region is far below the average for metropolitan areas. When standardized for industry mix, however, it turns out that the rate is as high as you would expect it to be. The question arises, therefore, do these women represent a potential supply or not? Why are not female-labor-using industries attracted by the surplus? Wages aside, there are at least two other factors relating to the character of the dominant industries which influence the outcome.

The first is the dispersion within the region of the plants of our dominant industries. The ratio of central city employment to SMA employment in manufacturing is much lower in Pittsburgh than in any of the large SMA's. Outside the Central City, the gradient in Pittsburgh is also flatter. The reasons are obvious. Our industries could not be piled up one on top of another as in the garment district even if our land were flat. The topography encourages still greater dispersion. But the importance of this for our purposes is that the early dispersion of manufacturing (plus the dispersion of mining) led to a dispersal of population which is also unmatched among our large SMA's. To the extent that pools of female labor are relevant to industrial location, Pittsburgh is at a disadvantage because a greater radius is required to form a pool of a given size. One must bear in mind that the areas in which the plants of the dominant industries operate are not exactly the most desirable as sites for other kinds of industry. (We shall return to this point later on.)

The second point has to do with the work schedule of the man in the family. Steel is a three-shift industry. The typical worker is not assigned to a particular shift for an indefinite period. Instead he works from 8:00 to 4:00 for some time, then 4:00 to 12:00 for some time, then 12:00 to 8:00 for some time. He also has to put in his share of weekends. It is reasonable to suppose that under these conditions the housewife is somewhat less willing to work than under ordinary conditions. Taken together, these factors tend to dissipate some of the labor force advantages we normally attribute to SMA's. And they are consequences of characteristics of the dominant industries.

Land. We normally assume that an SMA is large enough in area to nullify any considerations of site availability as a location factor except for industries with very special requirements like steel and chemicals. In general, I think this is a sensible approach. Nevertheless, I feel compelled to attach some importance to the impact of an industry's operations on the quality of the air and water in the surrounding area. Pittsburgh, as you all know, was notorious until recent years for its smoke and dust. There were three causes for this condition. The principal one was the use of soft coal as fuel in households and industry. A second was the steel industry. And a third was the railroads with their steam

engines. All this has changed now and I do not mind using this forum
as an opportunity to plug the radical improvement in the quality of
Pittsburgh's air. A white shirt will now stay white in Pittsburgh for as
long as it will in any city in the country. But it will take some time
to work off our reputation. And furthermore, at a time when the reputa-
tion was founded in fact, it was bound to exercise a restraining influence
on the growth of subsidiary industries in the region.

Intermediate Goods and Services. So much for the primary factors
of production. I said earlier that location analysts have paid attention
to a dominant industry's impact on the location of other industries
which are oriented to the supply of the product of the dominant in-
dustry. But agglomeration is nourished more by the availability of a
wide range of goods and services created in the first instance by the
growth of the dominant industries. Transportation is the classic illus-
tration of this phenomena. One industry attracts the service, and a
second industry coming in finds that the service is available at costs
which are lower than they would be in virgin territory. The second
industry also finds already in existence a whole community of suppliers
of business services such as legal, accounting, duplicating, etc.

The question I raise is whether the emergence of these services and
their availability to other industries depends on the character of these
industries which trigger development in the first instance. I think much
depends on the internal organization of these industries. Large firms
incorporate many of these services within their own operations because
they can achieve scale economies within the firm. They are much more
fully integrated and therefore depend less on outside suppliers. On the
one hand, this means that, dollar for dollar, their business is less of a
stimulus to the creation of a community of independent suppliers. On
the other hand, the new entrant is not likely to find that the company
is anxious to spread its fixed costs by making its services available to
outsiders.

Again, consider the classic example of external economies: transport
services. A firm which operates its own truck fleet on an exempt basis
is specifically forbidden by the ICC to transport freight as a common
carrier. Imagine then that you have two communities of equal size. In
one of these, all the firms rely on common carriage. Hence service to
and from a wide variety of places is available to all comers. In the other
community, every firm has its private truck fleet. True, the roads are
built and this helps a lot. But there is no service available to the new
firm coming in unless it starts big.

We do know that Pittsburgh is not up to par in employment in
ancillary services. This is indicated by a calculation of location
quotients based on the 1950 Census of Population. The Duncans in

their recent book, *Metropolis and Region,* also found that Pittsburgh had less than the national average per capita employment in service industries broadly defined. Only Detroit among the SMA's of 1,000,000 population or more shared this characteristic with Pittsburgh. It goes without saying, that much of my reasoning is applicable to Detroit as well.

Summary and Conclusions. It should be apparent by now that what I am reaching for is the specification of a function which relates external economies and diseconomies to industry structure, size being held constant. My feeling is that we have been too prone to associate external economies and diseconomies with size. We have been disturbed at not being able to derive a satisfactory correlation between the two. What I have tried to do is explore some of the residual variation around the size function. I recognize the difficulties of adequately formulating and testing these notions. But I do not think we can afford to ignore them because they are difficult if, as I maintain, they are relevant to an understanding of the dynamics of area development.

To come back to my first point: I think we are not using the optimal combination of tools in regional analysis. We know we can do a lot more to refine the methods we use to trap what I have called the demand side of interdependence. We need bigger and better regional input-output tables, regional capital coefficients, regional consumption functions. But we are not equating marginal returns in all directions if we do not, at the same time, push vigorously on the supply side of the problem.

I said we need new tools in regional analysis. I am prepared to modify that statement in favor of this one. We need to make better use of some old tools which we have not yet applied very extensively to regional analysis. We need to work out the regional implications of market organization.

ECONOMIC QUESTIONS IN URBAN REDEVELOPMENT

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America's experience with federally-supported urban redevelopment has been quite limited. The great bulk of the present program is still in the execution and planning stage. Yet, short as experience has been, there have already appeared very many riddles and more than a few misconceptions. Those who have given time and thought to the rationale of city planning find themselves troubled by recurring questions on the means and goals of urban redevelopment. In this paper I will touch upon only three of these questions, although you have my complete assurance that the list of perplexities is much longer. These three deal with (1) the nature of subsidy, (2) induced capital formation, and (3) some problems connected with the pricing of redevelopment facilities.

I. *The Nature of Subsidy*

There is a prevailing notion that, because urban redevelopment usually requires a public outlay, there is also necessarily a subsidy either to the developer or to the occupants of the new real estate. Thus there has been an outcry from the press and the public that it is morally wrong to subsidize hotels, office buildings, or luxury apartments on urban renewal sites—the now notorious $8,000-a-year penthouse in one New York City project comes to mind. In consequence, strong pressure is exerted to confine land write-down aids to housing, particularly to so-called "low-income" or "middle-income" housing.

Few economists would agree with the view that every public outlay for a private venture represents a subsidy or, while we are on the point, with the widely-held corollary view that there is no subsidy unless there is a budgeted public outlay.

A subsidy is defined as a negative tax. A tax is not a tax unless there is an unrequited transfer of private resources to the public treasury. Obviously, a payment to government is not a tax if the payer receives at least an equivalent return of goods or rights; i.e., his balance sheet remains unaffected by the transaction, as when a private dealer purchases Treasury securities or when a household is assessed for a sewer which adds at least equal value to the resale price of his dwelling. And, to determine the incidence of a tax, we must look not to the payer of record but to the person who has suffered a shrinkage in income, consumption, or net worth.

By the same token, public outlays for urban redevelopment will not constitute a subsidy to private parties unless it can be shown that the latter have been made economically better off. Putting aside some subtleties, chiefly with respect to consumer surplus, it is legitimate to conclude that when a private user pays the going market price or rent for possession of real estate, he has received no subsidy regardless of how large may be the public expenditure associated with the transaction.

I can best illustrate this point by taking an example outside of the federal urban redevelopment program. Some of you know of the current plan of a number of communities in New Jersey to reclaim a large tract of swampy meadowland which is located close to New York City and which has considerable potential as a site for industry. Let me assume that draining and diking one hundred acres of this marshland will entail a cost of $3.00 per square foot or a total expenditure of roughly 13 million dollars. Let me further assume, not too unrealistically, that in a free and open auction to industrial developers the reclaimed land will command a price of $1.50 per square foot or 6.5 million dollars. That is, given active bidding by informed investors who have evaluated the net earnings potential of the reclaimed tract relative to competitive opportunities, $1.50 per foot is deemed to be the maximum price which just makes the purchase worthwhile. Clearly, neither the successful bidder nor his future tenants have been given a subsidy notwithstanding a net community outlay of 6.5 million dollars. The community outlay represents an expenditure made necessary by a physical defect in the site for which nature is to blame. If there is a subsidy, it is to the community-at-large which expects to derive widely-disseminated benefits from new industry. The community is, of course, free to decide whether these social gains are worth the social costs.

The economics of reclaiming slum land are not different from the economics of reclaiming swampland. In both cases, public assistance may be needed to bridge the gap between costs and re-use value and the expenditure of public resources is (or should be) justified by anticipated social benefits. Condemnation awards for even the most deteriorated real estate are usually generous and the costs of tenant relocation and building demolition are far from negligible. To be sure, if a site has very great promise, it is not at all impossible that private developers will offer a price equal to (or, for that matter, higher than) the full costs of acquisition and clearance. In this event, urban redevelopment can proceed without public outlay. In the case of most redevelopment projects, however, the income potential of the replacement real estate will not warrant full payment for the site. And the cost-value gap may be further widened if the urban renewal agency

specifies something less than the highest economic use of the site (e.g., town houses instead of apartment houses or apartment houses instead of office buildings), or if it insists on higher standards (e.g., larger amounts of open space) than tenants are willing to pay for by higher rent. A public outlay then becomes obligatory to close the spread between costs and market value. But as long as the highest market value is received on resale, the public grant for land writedown will not be a subsidy to the developer or to his tenants, however much a subsidy it may be to the community-at-large. The presumptive purpose of public outlay is to overcome a cost disadvantage, not to give a price advantage.

II. *Induced Capital Formation*

If these conclusions on the nature of subsidy are correct, we can next turn to a second question of some importance: To what extent will publicly-aided redevelopment prove a tool for accelerating economic growth; i.e., for increasing the nation's stock pile of productive capital facilities—housing, factories, office buildings, and so forth? Or will public action succeed in merely redistributing a given volume of investment; i.e., in changing the geographic location of capital facilities that would have been built in other places?

Many of those who most enthusiastically support a massive urban redevelopment program seem to believe that public outlays for slum clearance and rehabilitation will result in a large volume of additional private investment. Certainly, urban redevelopment stands high on the list of those who wish to step up our economic growth. At the same time, many of those who fear a bigger program because of its inflationary consequences must also, at least implicitly, subscribe to the notion of accelerated investment. Since government outlays for the acquisition of slum land represent a nonexhaustive payment (i.e., a transfer in ownership of existing assets rather than a claim against current production), the objections of the nonenthusiasts would appear to be based principally on the expectation that slum clearance will bring forth an excessive volume of private capital formation.

Both hopes and fears may be groundless. They derive from the fact that, to date, each dollar of public outlay for redevelopment has been accompanied by several dollars of private investment. To take a hypothetical but representative project, a municipality acquires and clears ten acres of slums, in or near the center of the city, at a cost of 8.7 million dollars. The cleared site is sold at open auction for 2.7 million dollars to an apartment developer who proceeds to build a 24 million dollar apartment project. A public expenditure of 6 millions (a 4 million federal outlay and a 2 million municipal outlay) is thus accompanied by four times as much private capital formation. More-

over, additional private investment may take place adjacent to the redevelopment site—retail stores or, now that the character of the area has changed, other new apartment houses, *ex post,* a total of five, six, or more dollars of private investment will be attributed to each dollar of public investment.

Plainly, we cannot accept, uncritically, this narrative as evidence that the multiplier effect of public urban redevelopment is anywhere near the size indicated. As Leo Grebler and others have already pointed out, there are a number of reasons for believing that urban redevelopment will stimulate only a moderate amount of incremental investment.

A net increase in aggregate investment could be attributed to urban redevelopment only if it were demonstrated that the new real estate improvements, or the act of producing them, would cause space users (households and firms) to enlarge their aggregate expenditures for building space; or better yet, only if it were demonstrated that consumers were induced to hold more structure capital per unit than would have been the case in the absence of a public redevelopment plan.

Such an increase in, say, residential capital formation could arise from several circumstances. (With minor substitutions, chiefly on the relation between site efficiency and profits, my arguments would apply also to business demand for commercial and industrial space.)

Forced Spending. Since the presence of substandard and obsolete structures is usually a precondition for the selection of an urban redevelopment site, slum clearance results in a shrinkage of the aggregate inventory of cheap housing space. Although some of the dislocated households will dissolve, we can observe from actual surveys that most find substitute locations, generally at higher rents; i.e., willingly or unwillingly, they are forced to hold a larger amount of residential capital. We can conclude, therefore, that to the degree that urban redevelopment accelerates the demolition of old structures, the resulting increase in the national rent bill is a factor making for a higher rate of capital formation.

A Better Accommodation to Existing Demand. Because locations are differentiated, i.e., because each location has some unique characteristics, it is conceivable (and even probable) that a redevelopment project will not only attract customers away from other sites for new housing but will cause additional tenants to enter the new-housing market; i.e., to upgrade their present housing. For example, practically every neighborhood, even the poorest, contains residents who are underhoused with respect to income and preferences but who have been unable to find suitable quarters in their present neighborhood where, for sentimental or business reasons, they insist on remaining. Cases in

point are local political leaders, storekeepers, doctors, and others with a strong desire to live close to their employment. Of course, such effective but unsatisfied demand is a rarity in the American economy, and it is proper to ask why unaided private developers have not sought to accommodate it. The answer is that the real-estate market is far from perfect. The supply curve for one of the critical inputs—land—is quite discontinuous. The acquisition of a site in a highly developed area involves many parties at interest and many legal complexities. The unaided private developer who tries to piece together an acre of land at a predetermined point in the city finds it an arduous and often unsuccessful task, inasmuch as it may entail negotiations with twenty owners and an equal number of lessees having various legal rights. Each of these parties at interest can seek to exact a monopolist's price or flatly reject any and all offers. To be sure, even under such adverse circumstances most builders can assemble a limited amount of plottage. But the market prospects of a small-scale development surrounded by an expanse of slum and blight are not often promising.

With urban redevelopment powers, such impediments to land assembly are readily overcome. Through eminent domain the municipality can acquire sites of almost any size and can therefore create a neighborhood environment with maximum market potential. It is reasonable to assume that, price aside, a large-scale project in a protected environment will not only win tenants away from competitive sites but may also generate new demand.

Price. Finally, urban redevelopment could lead to an appreciable increase in aggregate demand if, as a result of land writedown, the new real estate were offered at a bargain rent. However, as was seen, the economic rationale of the federal land writedown program has been to eliminate a cost disadvantage rather than to confer a price advantage. The resale price of a site is set by an open auction so that the amount of land writedown given will be no more than is necessary to achieve a competitive market rent. To be sure, there may be economies of scale in building and operating a large-scale project. But, presumably, the extra profit potential is already reflected in the winner's bid price. In short, as the urban redevelopment program is presently conceived, we cannot, in principle, place much weight on bargain pricing as a means for stimulating additional demand. The subject of pricing is so important, however, that it will be discussed more fully later on.

Summarizing, as long as the objective of urban redevelopment aids is to eliminate cost disadvantages rather than to subsidize occupants, new investment demand will be generated chiefly through forced spending and through product differentiation. It is not possible, of

course, to make a priori estimates of the market response to these two forces. But, in my opinion at least, this response will not be very great. Public pressures to limit tenant dislocation and forced spending are, to put it mildly, overwhelming. And, as in other markets, product differentiation without price advantage tends to result mainly in inter-product shifts in demand rather than new demand. This is particularly true in the market for real estate where the existing product is seldom scrapped but lingers indefinitely on the market, offering powerful price competition to the new product.

Most likely the principal accomplishment of urban redevelopment will be to alter the locale of investment rather than to induce new investment. Let me emphasize, however, that the redistribution of urban investment is a gain hardly to be scorned. In fact, given a pervasive tendency toward population decline in both large and small cities, the paramount urban need may well be a more satisfactory rearrangement of residence and industry rather than a substantial increase in the aggregate real-estate inventory.

III. *Pricing Problems*

Land Writedown versus Rent Writedown. Turning next to some practical problems in pricing, let me refer once more to the hypothetical apartment project described earlier in this paper—the one which required a land writedown of 6 million dollars. Such a write-down was needed, it was said, to cover the difference between cost and market value. Given the specifications of the plan, had the private developer paid full costs for the site, he would have had to obtain a rental of approximately $56.00 a month per room. In the judgment of all concerned, such a rent was deemed to be $6.00 above the going market rent of $50.00 a room. A land writedown grant was made to wipe out this $6.00 handicap.

But according to this analysis the term "land writedown" is surely a misnomer for the term "rent writedown." The real object of public aid is to bring rent down to a competitive market level. Obviously, a rent reduction could be achieved by writing down not the cost of land alone but any of the capital or operating costs upon which rent is based. And, as a matter of financial arithmetic, land writedown is hardly the most effective way to reduce rents to market level. In fact, an aid program restricted solely to land writedown will be an unnecessarily limited program. This conclusion follows from the fact that land costs, even when high, do not ordinarily have a major impact on rents. Thus, in the case of our hypothetical apartment project, a 70 per cent writedown of land costs from 8.7 to 2.7 million dollars succeeded in reducing rents by little more than 10 per cent. Had the site been

conveyed free of charge (i.e., had there been a 100 per cent land write-down), the lowest rent attainable through land writedown would have been approximately $48.00 per room, a total reduction of 15 per cent.

Suppose, now, that in a given redevelopment area the lowest rent which can be attained after a 100 per cent writedown is substantially higher than what tenants will pay. Then, either the redevelopment plan must be revised or some more powerful form of aid will have to be given. What can we suggest? An obvious proposal is that the present federal land writedown formula be abandoned in favor of outright cash grants in whatever amount is necessary to bring rent down to market level. However, other tools can be thought of. One of these is the more imaginative use of liberal FHA Section 220 mortgages to reduce debt service charges (and therefore the rent) of redevelopment housing. A much bolder step in the same direction is New York State's Mitchell-Lama Law. Under this law the builder of a (limited profit) private housing project can obtain from the state or municipality a fifty-year, $3\frac{1}{2}$ per cent mortgage equal to 90 per cent of costs. He can further obtain from the municipality a 40 per cent abatement in real estate taxes. Applying both of these aids to our hypothetical apartment house would have further lowered rent from $50.00 to $32.00 per month per room—a rent writedown three times as great as that which resulted from land writedown.

Our urban redevelopment programs could be immeasurably improved by adopting rent writedown tools in substitution for, or in supple-mentation of, land writedown grants. Were municipalities to evolve a Mitchell-Lama type formula, three important advantages would accrue.

First, because the rent-reducing power of municipal tax and credit aids is so great a municipality is enabled to redevelop (assuming that it were wise to do so) high-cost–low-value neighborhoods; i.e., highly unfavorable slum areas where even a 100 per cent land write-down is insufficient to bring rent down to market level. A municipality could thus widen its program by choosing neighborhoods which are now out of the economic reach of a land writedown formula. In many cities such neighborhoods may be precisely the ones where it is most urgent to encourage private investment or to bring in a different class of population; e.g., families with school age children to areas with under-utilized schools and white middle-class families to neighborhoods which are tending toward racial and social imbalances.

Second, because cash land writedown grants could in many instances be dispensed with, a municipality would be able to carry out a very considerable amount of redevelopment outside of the federal program. This is a gain not to be lightly dismissed, since the municipality is set free from the delays and uncertainties of Congressional action and federal control.

Third, armed with a combination of tax, credit, and land writedown aids, a municipality could, if it so desired, inaugurate a pricing policy which went beyond the mere elimination of cost disadvantage; i.e., it could engage in outright subsidy. By setting rents below the going market level, a municipality could then tap additional layers of housing demand and thereby help overcome a housing shortage, should one exist. The extra volume of housing investment thus obtained would, of course, depend upon the community's demand elasticity for housing with respect to price. While new demand could be generated by bargain prices on any quality-class of housing, luxury as well as nonluxury, there is reason to believe that consumer response per given amount of rent reduction would be greatest if subsidy were limited to so-called "middle-income" housing. Admittedly, a program of housing subsidy would be laden with very many dangers and inequities. A full discussion of merits and pitfalls would however carry us too far afield.

Some would attribute to tax and credit aids still a fourth advantage; namely, that, unlike cash writedowns, no government costs are involved. That is, since the municipal loan would be repaid and since the tax yields on the new real estate, even after partial tax abatement, would usually equal or exceed the taxes paid on the former slum site, it is believed that the public escapes a burden. I do not have space to debate this point. Suffice it to say, that a fifty-year, $3\frac{1}{2}$ per cent loan is an extremely scarce economic good—a privilege for which the private market would gladly pay a price. For example, if the municipality would permit me to replace my twenty-five year, $5\frac{1}{2}$ per cent home mortgage with a $3\frac{1}{2}$ per cent loan for an equivalent term, I would be willing to pay a premium (in cash or additional real estate taxes) of up to $240 per $1,000 of loan. The revenue foregone through the use of municipal credit and tax aids—quite apart from the cost to the federal treasury of additional tax-exempt municipal bonds—constitutes a public cost, whether or not it appears in the budget. Whether such aids constitute a subsidy or not depends on how they are used.

Rationing Public Aids. My final thought is that, unless a deliberate policy of subsidy is embarked upon, the amount and type of aid should be strictly rationed in accordance with the cost-value equation of each redevelopment project. If public resources are not to be misused, no more or less aid should be given than is required by the market to fulfill the plan. For example, slum site A in the central core may be an area of great potential market appeal. An analysis may indicate that 1,000 new apartment units could be rented at $60.00 a room and that such a rental would be sufficient to meet all the costs of redevelopment. Here the only aid which should be given is land assembly. Slum site B may be an area of moderate market appeal, with a rent potential of say $40.00 a room. Such a rent might be attained solely

through the use of a long-term, low-interest mortgage. By contrast, slum site C is an area in which not only are site costs high but which is also so low in status that it would be difficult to attract tenants unless rents are set appreciably below the minimum market rent for new housing in the city as a whole. That is, to attract the required number of occupants a rent of $20.00 per room may have to be offered, perhaps $15.00 less than the next best opportunity in the new-housing market. In such cases, it may be necessary to mobilize the full panoply of public aids.

The urban renewal administrator should also recognize that a project is underpriced if, at the predetermined rent, demand exceeds the available supply; i.e., if more tenants of the wanted type apply than there are available units. An oversubscribed apartment project indicates that more aid has been given than is necessary for the success of the plan and that the tenants (or developers) are receiving what may be an unintended subsidy. Conversely, weak demand for a project would be evidence that a still lower rent and a larger dosage of aid is needed if the objectives of the plan are to be reached. Incorrect pricing not only means an overuse of public resources or a wastage of private investment, but it may also thwart the community's redistribution plan. Thus, putting $25.00 per room housing in a popular $60.00 per room neighborhood could significantly weaken the chances of attracting families to neighborhoods of much lower status. When orchestra seats are made available at balcony prices, balcony seats tend to go begging.

Let me close by repeating that the few points touched upon in this paper barely scratch the surface of deeper quandaries to which our urban redevelopment program has given birth. How does one balance economic and noneconomic factors in selecting a site and in choosing the optimal type of replacement real estate? What yardsticks should be applied in determining whether the benefits of a given redevelopment scheme justify its costs? And what principles can be established (welfare economists, please note) for the compensation of dislocated occupants?

Unless urban redevelopment policy obtains a prominent place on the economist's agenda, it will be shaped in accordance with the wishes of special-interest groups. We will then be giving credence to Henry Wallich's remark that just as experience is another word for mistakes we have already made, policy is another word for mistakes we are about to make.

DISCUSSION

BARBARA R. BERMAN: Professor Tiebout's paper gives a new and improved prescription for the ingredients of a general equilibrium model of intra-urban location. He would throw out the transport surface in favor of a more realistic cost pattern; he would take account of externalities; and he would elaborate on the effect of governmental decisions on the pattern which emerges.

It would be hard to quarrel with any of these suggestions, so far as they go. To take the last point first, his discussion implies that a single projection of the intra-urban location pattern is useless. What is required is a different projection for each possible set of governmental actions, with the politicians being allowed to choose the one they think most popular in some sense. The proposition that the treatment of government actions as rigidly exogenous to the system is bad practice is true even in the unlikely event that the projection was not explicitly undertaken for the guidance of policy-makers.

As to externalities, it can be argued that the effect of the government action is at least partially to internalize them, in the sense that firms which produce external economies are rewarded and those which produce external diseconomies are penalized if they do so. As an example of the latter, zoning laws place artificially infinite rents on sites which would otherwise be eligible; thus forcing a firm to move to a less eligible site and to curtail the external diseconomies it would produce if left to itself. To the extent that externalities are internalized, private optima add up more nearly to social optima. The government fixes the game so that what is good for General Motors *is* good for the country.

When all this is said and done (and easier said than done, of course), we are left with a model—an improved one, to be sure—in which everybody, or all the newcomers in a given period, or, to generalize, a large number optimize simultaneously. I should like to submit that this type of model is inappropriate as a description of the urban location process unless used in a particularly conservative way.

The market for the services of land in an urban area does not operate like Marshall's grain market. In the former, buyers cannot transfer their custom from one seller to another without substantial cost. But it is the possibility of some form of quick and costless recontract which is at the heart of any valid comparative statics approach.

The crops sown on a piece of land can be changed from year to year at relatively small cost, so that one would expect that the method of comparative statics would work quite well in predicting the allocation of agricultural land. But in the urban land market, the order in which buyers attach themselves to particular sellers is crucial. If firm A, with poor or limited foresight, is choosing a location today, it will optimize with respect to the present situation. Firm B, coming along some time after A is established will do the same. Now it may be that the locations of A and B should have been interchanged in the light of the situation after both are established, but the cost of the move will intervene to prevent this from happening.

In the Koopmans and Beckmann article cited by Tiebout, in which the location assignment problem is set up as a problem in linear programming, the authors have this to say about the practical applicability of their model: "Whether a competitive market can find an optimal assignment through a process of alternating adjustments in prices and in choices of location cannot be answered without specifying dynamic characteristics of the market processes in question. [But] there is a possible parallel between the iterative computation methods for the transportation problem . . . and the market adjustment processes. . . ."

This, I think, is rather egregious. A great gulf of dollars lies between the cost of an arithmetic exercise and the cost of achieving an optimal location pattern in period t, when the particular solution arrived at in period t-1 has turned out to be nonoptimal.

If we look, for example, at the present arrangement of residential areas in a city like New York, it is obvious that if there is any optimizing occurring, it is short-run rather than long-run optimizing. In all truth, how can we expect optimizing with respect to the longer run if the economists themselves have yet to provide projections based on an intellectually respectable model? This might be labeled "the vicious circle of poverty in urban economics."

Thus the urban location problem is, I think, most appropriately tackled by an explicitly dynamic model, or by a series of simultaneous-determination models, each covering a short period. If we are asked the question, "Given that the government will do so and so, and that activities will be carried on at such and such a level, what will the city look like ten or fifteen years from now?" we must, I believe, first answer the question, "What will the city look like one or two years from now?" To answer the latter question may require that we make explicit year-by-year projections of activity levels, and this may be asking too much. So perhaps I had best ask for moderation in quantity rather than a change of quality by restating the point as follows: Great long leaps into the future on the basis of single optimizing models are to be avoided. Just how short the leaps had best be is, I suppose, a matter for practical judgment.

I think that the very perceptive suggestions made by Professor Chinitz also cry out for dynamic treatment. In comparative statics, the situation is thought of as being created anew each time an exogenous variable or a parameter is changed, and the system must be re-solved. Dynamic analysis is needed where the dead hand of past solutions lies heavily on the future. Chinitz hypothesizes—and correctly it seems to me—that lack of diversification tends to lead to more of the same. This is the dead hand of the past, and with a vengeance. If risk-takers are needed to breed more risk-takers, then the supply of entrepreneurs is a "population" problem of a peculiar sort, again to be approached dynamically.

It may be argued that dynamic models are harder to construct than static, or that we cannot begin to fashion dynamic models until we have a static model of some believability. But for practical purposes—and I believe both Tiebout and Chinitz are concerned with such purposes—a crude dynamic model may be better than a highly tooled, multijeweled static creation.

BRITTON HARRIS: The three papers in this discussion apparently deal with a very diverse group of subjects in the field of urban economics. Actually, however, they appear to me to have a common thread and one which requires careful identification. Properly identified and brought into the open, this common element will provide many analysts with subject matter and theoretical problems for years to come.

Each in a different way, these three papers seem to be coming to grips with the problem of the urban metropolis as a whole, viewed partly, if not entirely, in economic terms. This effort to deal with a whole leads to a number of different discoveries. Winnick apparently has in mind, but does not discuss, an over-all view of the desirability and needs for redevelopment in certain parts of our metropolitan areas as a benefit to the community as a whole. The effect of his paper is to focus on this undiscussed question by way of clearing the underbrush. Chinitz has identified a number of behavioral problems which appear to affect all aspects of the functioning of the urban economy but which arise out of social and institutional patterns common to the city as a whole rather than out of purely economic considerations. Tiebout gives a look at the opposite side of Chinitz' coin, using somewhat the same techniques as Winnick. Instead of focusing on the total reaction of the urban economy to growth and change, he focuses on the patterns of internal distribution. His major contribution appears to be again that of clearing some of the underbrush and making more clear the exact nature of the problems which we will face in pursuing his suggested analysis.

I should like to draw from these excellent papers and from my own recent experience some of the main indications which I see for future research in this field. At the Institute for Urban Studies at the University of Pennsylvania and the Penn Jersey Transportation Study, I have been deeply concerned both with the external economic ties of metropolitan areas and more recently with the analysis of the internal distribution of population and economic activity in the Philadelphia metropolitan area. Conclusions from these experiences may help to bring into a sharper light some of the implications which are inherent in the remarks of the three speakers.

Clearly we are dealing with a problem which, if it is to be treated as an economic one, requires a broad interpretation of the term "economic." The behavior of an urban complex is full of institutional and sociological aspects which some have tried to define out of the realm of economics. However, there is more than poetry in the peculiar history and flavor of different urban areas. These subtle differences, which Chinitz has tried to define for New York and Pittsburgh, find expression in economic behavior and deserve our careful analysis and consideration. Our first difficulty is to define and then to quantify the economic effects of these phenomena.

Second, the growth and change of a metropolitan area appear to depend on the behavior of three markets for land, labor, and capital (to which Chinitz would add entrepreneurship) which are, to say the least, imperfect and not clearly understood. The market for land in particular is nonhomogeneous and sticky. In consequence of all these imperfections, equilibrium theory can perhaps indicate directions of change, but it is powerless to deal with rates of

change and, of course, does not yield a dynamic theory. Yet the problems to be investigated are embedded in questions of change and growth.

Third, when we examine the question more carefully and attempt to build any part of a satisfactory theory, we find that the magnitudes involved have been inadequately surveyed and that the results of these surveys have been tabulated in ways which are frequently unsatisfactory for analysis. It appears that in order to explain and predict change in the metropolitan framework we shall have to go back to a much deeper analysis of individual and family behavior, consumer choice, and business decision making. Most of the observations which have been made of families of consumers and of business have not been made in a locational framework and have not been analyzed or aggregated in a meaningful way for locational analysis. I would venture to suggest that there is a whole field of analysis based on the individual unit of observation and making use of probabilistic concepts which has barely been scratched to date but which will prove necessary before these problems will yield their secrets.

Fourth, when we look at the urban metropolitan region, we readily perceive that its growth and change are the result of the interaction of a large number of private and public decisions. If we study a particular problem such as urban redevelopment, industrial labor market, or locational trends, we must make assumptions of *ceteris paribus* which are wholly unjustified, because of the interaction of the problem which we are studying with the environment. This means in my estimation that the economists will have to co-operate with operations researchers in picturing the metropolitan area as a total system. In view of the complexity of the system, this will be a most difficult task. Not the least of the complications is the fact that "externalities" and mass effects such as have been discussed in two of these papers lead to gross nonlinearities in the formulation of the system. This adds greatly to the difficulties of mathematical formulation and emphasizes the differences between this and many other economic problems.

Finally, as Tiebout has aptly emphasized, the role of public decision making is of very great importance in metropolitan areas, especially as it affects the technology of transportation, the land market, and possibly the industrial locational environment. I would suggest that his definition of this problem is perhaps somewhat narrow and that the effects of public policy are much more diffuse and diverse than he recognizes. At present, public decisions appear to be made with no more than an intuitive grasp of the consequences which will flow from them through devious channels of private decision making. At the point where public decisions are made, we probably leave the realm of economics for good, but it is likely that the type of economic analysis suggested by our three speakers will ultimately be needed to spell out the consequences of alternative policies in a situation where quite apparently intuition no longer suffices to provide adequate understanding.

Irving Morrissett: I would consider "the nature of subsidy" in a rather different light than that explained by Mr. Winnick. He defines a subsidy as a negative tax. I would agree that a "tax is an unrequited transfer of private

resources to the public treasury," but not that a subsidy is an unrequited transfer of public resources to a private treasury. A subsidy is more usefully defined as payment made by government for the purpose of persuading or making it possible for the recipient to do something he otherwise would not do. The subsidy is usually conditional upon the performance of the specified act: the farmer must grow or not grow certain crops, the shipowner must continue to run ships, and the urban redeveloper must offer facilities below cost or undertake some other act that would not be dictated by market forces. Taxes are sometimes levied for the purpose of persuading people to do something they otherwise would not do—the high liquor tax, for example, is intended partly to discourage riotous living. But taxes usually are collected for other purposes, such as redistributing income (we refer to the opposite of this as a "transfer payment," not a subsidy) or paying for government activities.

In my definitions, a subsidy is efficient if it gets the job done—that is, if it is successful in modifying the action of the recipient as desired—without paying more than is necessary to modify his actions. It is "excessive" if more than the minimum necessary to modify his actions is paid. Mr. Winnick's definition of a subsidy corresponds to my definition of an excessive subsidy; in his lexicon, there is no subsidy if the payments made are just sufficient to get the job done. But this is peculiar terminology. It is common gossip, well founded I believe, that the urban redevelopment program involves subsidies. The subsidies are given to a local government or redevelopment authority in consideration of the carrying out of a certain approved plan. The subsidies do not go to private parties, because they are directed toward the objective of modifying the behavior of the local authority. The effects of the approved plan do, however, carry beyond the original federal-local relationship. The redevelopment plan specifies land uses that are to be permitted or encouraged; and although the original subsidy may not make a $10,000 penthouse available at a bargain rate of $8,000, it cannot be divorced from responsibility for making an $8,000 penthouse available at a location where it otherwise would not be. The original subsidy usually is a necessary condition for setting in motion a comprehensive plan, the results of which must be judged as a whole.

I am uncertain about the meaning of Mr. Winnick's assertion that the purpose of a subsidy is to eliminate a cost disadvantage. I can understand that a redevelopment plan typically reduces the cost of using particular land for more or less specified purposes. But the reference to elimination of cost disadvantages sounds very much like the concept of "cost equalization" which has so often derailed intelligent consideration of tariff and public utility problems. The questions of "what costs" and "whose costs" could lead analysts into great and unnecessary confusion. The simpler and relevant question is: How much of the cost must be met by subsidy in order to get responsible recipients to carry out the agreed-upon plan?

The point of the third section of Mr. Winnick's paper is well taken: there is no compelling reason for confining the form of urban renewal grants to defrayal of land acquisition costs. There are two essential contributions that government makes to urban renewal. First and most important is eminent domain coupled with a comprehensive plan. The second is subsidy. As Mr.

Winnick points out, both the scope and flexibility of urban renewal plans could be increased by making wider use of tax and interest rebates as well as land writedowns.

The middle section of the paper deals with one aspect of what is the heart of the urban renewal problem—the heart being the value of the benefits derived. Subsidies have two general purposes. One is to substitute public value judgments or "tastes" for private judgments, as in the subsidization of symphony orchestras. The other purpose is to encourage the production of goods and services which benefit third parties. If a subsidy is involved, such indiscriminate, nonmarketable benefits usually constitute only a part of the benefits of production, as with the certain private benefits and assumed public benefits of our ship-building subsidy program. If all of the benefits of production are indiscriminate, as with national defense and city parks, the activity is usually socialized and there is no subsidy in the sense in which I have used the word.

From among the many possible indiscriminate benefits of urban renewal, Mr. Winnick has selected induced capital formation and shed light on some of the most important analytical problems related to it. If new capital is only diverted from one location to another by a renewal program, there is a gain only if benefits are calculated for an area that excludes the location from which the investment project was enticed. There are several routes by which net additions to capital formation may come, as Mr. Winnick points out. The sheer destruction of low-grade and obsolete capital goods—industrial, commercial, or residential—may induce replacement which otherwise would have occurred much more slowly. The same result can be obtained with a well-placed bomb, and in both cases the human as well as the monetary costs of destruction must be weighed against the benefits of the increased capital.

A second kind of capital formation attributable to urban renewal, as suggested by Mr. Winnick, is encouraged by the availability of new construction sites of types not previously available to meet the needs or desires of prospective investors. Still another type of real capital gain, if not of capital formation, which Mr. Winnick might have added is the increase of efficiency in the use of existing capital. A city exists for the purpose, among others, of facilitating the production of a wide variety of goods and services. It can perform its facilitating functions poorly or well. Presumably these functions will be performed better as the result of a redevelopment program which would mean greater efficiency in the use of both private and social capital.

The difficulties of assessing the magnitude and impact of induced capital formation are illustrative of the problems encountered in assessing the whole range of possible benefits of urban redevelopment. The monetary costs of redevelopment are fairly ascertainable, and the nonmonetary costs are identifiable if not easily measured. But on the benefit side, both the monetary and the nonmonetary effects are for the most part quite difficult to identify and measure. The benefits of urban redevelopment constitute a large and important underdeveloped area of urban research.

PUBLIC UTILITIES AND TRANSPORTATION

FULLY DISTRIBUTED COSTS IN UTILITY RATE MAKING

By JAMES C. BONBRIGHT
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Nature and Importance of Full-Cost Apportionments

Writers on the theory of public utility rates have sometimes declared that when the rates charged by any utility enterprise must be made to defray the total costs of production even though the enterprise is operating under conditions of declining unit costs, each rate should be viewed as consisting of two components: a "price," to be set at the marginal cost of each type of service; and a surcharge or quasi-tax designed to supply some appropriate share of those additional revenue requirements that would fail to be covered if all rates were to be set at mere marginal costs. Even the surcharge would be related to marginal cost, despite careless statements to the contrary in some of the literature. But the relationship would not be a simple one and might well be deliberately "biased" or "tilted" by allowance for demand-elasticity factors. The same idea is implicit in the more popular but cruder assertion that rates should be set somewhere between out-of-pocket costs as a minimum and "value of the service" or "what the traffic will bear" as a maximum.

The import of this distinction between the price and the surcharge lies, of course, in the difference between the purposes which they serve in the multiple role of a price system. Insofar as the demand for the service is restricted by the price, by the minimum rate, this restriction is deemed desirable for reasons of optimum resource allocation. But insofar as the demand is also restricted by the surcharge, this further restriction (with qualifications which, however important, I shall here ignore) is deemed to be an undesirable side effect—an effect which may perhaps be minimized by skillful but limited resort to rate discrimination.

In actual practice, however, utility rate structures are seldom built up in this synthetic, two-step manner. Instead, when based on any comprehensive cost analysis whatever, they are usually derived analytically from the apportioned total costs of a utility business. With gas and electric companies, for example, overhead costs are typically apportioned by a functional-cost analysis which divides all, or nearly all, of such costs into those which vary with number of customers (cus-

tomer costs), those which vary with consumption of energy (energy or commodity costs), and those which vary with plant capacity and hence indirectly with maximum load or maximum demand (demand or capacity costs). In this way the cost analyst purports to determine what parts of the total cost of supplying a given community, say, with electric power represent the cost of the residential service, the cost of the industrial service, the cost of the street-lighting service, and so on through all of those classes and subclasses of service deemed important to distinguish for purposes of rate making.

Even the analysts who make these cost apportionments and who defend them on the witness stand in rate cases seldom offer them without qualification as measures of reasonable rates. Instead, they generally concede that rates may justifiably deviate from the imputed costs in deference to a variety of non-cost considerations. This concession goes to the point of recognizing the validity and compensatory character of rates for competitive services which fail to cover the very costs imputed to these services by the analysts themselves.

But there remains the question of what significance should be attached to these fully distributed costs even as guides, or even as points of departure, for sound rate determination, in view of their admitted failure to mark the dividing line between compensatory and noncompensatory charges, between profit and loss. And to this basic question the answers forthcoming from the analysts have been far from satisfying. The reply most frequently made is that cost is only one of several factors to be considered in the design of a rate structure. But while this assertion is entirely valid, it is also entirely beside the point. For the question at issue concerns the economic significance of the apportioned total costs, not the weight to be given to a specific cost that must be covered unless the service is to be supplied at an outright loss.

Mindful, perhaps, of the absence of any convincing answer to this fundamental question and mindful, also, of the notorious disagreements among the experts as to the most rational method of overhead-cost allocation—disagreements which defy resolution in default of any accepted objective standard of rationality—most state commissions have not made full-cost apportionments mandatory as a prelude to a decision on rate structure. Thus, in 1953 and again in 1957, when the Commonwealth Edison Company of Chicago filed an application for a general rate increase, the Illinois Commerce Commission declined to order such an apportionment despite the request of intervenors that the Company be required to submit one. In partial support of its refusal, the Commission referred to an exhibit, introduced by one of the Company officials, disclosing the existence of twenty-nine rival formulas for the

allocation of capacity costs alone—formulas each of which had received some professional sponsorship.

Nevertheless, I think it quite likely that, in the future, fully distributed cost apportionments will come to play a much more important role in rate regulation than they have played in the past. Already, indeed, the Federal Power Commission has relied largely on their use in the determination of wholesale rates for natural-gas pipeline companies; and this use is commanding increased attention on the part of the state commissions. At the present time, a committee of the National Association of Railroad and Utility Commissioners is engaged in a study of natural-gas cost allocations in the hope of securing general agreement among the state and federal commissions on acceptable allocation formulas. And I have been told that another committee of the Association is now considering the same subject as applied to the electric utilities.

In view of this situation, I believe that the rationale, or lack thereof, of full-cost apportionments has now become one of the most timely subjects in the entire field of public utility economics. Indeed, the subject may possibly take on critical importance because of the danger that commissions and courts may attach to the apportionments a specious economic significance which they do not possess—at least not in the present, primitive stage of their development.

Failure of the Sum of the Costs of Specific Services to Equal Total Costs

But what makes the philosophy of a full-cost apportionment one of such frustrating difficulty? The main reason, I take it, lies in the special character of the only costs that can be allocated, on a cost-causation basis, to specific quantities of specific types of service—all of which are supplied by a largely joint or combined operation. Of necessity, these allocable costs are differential or incremental or marginal costs. They are measured by the additional costs that must be incurred in order to supply the particular service in question; or, alternatively, by the costs that can be saved by ceasing to supply the service. But only rarely would the summation of these avoidable or escapable costs equal the total operating and capital costs of a utility business in gross.

In the textbooks on public utility economics, the most frequently cited reason for this failure of "the sum of the parts to equal the whole" lies in the assumed fact that most utilities are operating on the declining-cost portion of a unit-cost curve. Hence, their marginal costs of service—even their long-run marginal costs—are supposed to fall

substantially short of their average total costs. Within recent years, the validity of this assumption has been challenged with respect to many American utilities in their present stage of expansion. But the challenge is based on a priori grounds or on statistical data that seem to me, at least, to be far from conclusive.

Even, however, if the challenged assumption were found to be invalid and even if the company under review were found to be operating under conditions of constant or of increasing unit costs in the textbook sense of these terms, it would still be impossible, save perhaps for a new utility plant, to derive the allocable costs of supplying the various classes of service by an apportionment of the total costs. The reason for this negative conclusion is that the total costs which are used as the measure of corporate revenue requirements in a rate-level case are not of the same nature as the costs most clearly relevant to the determination of an economically sound rate structure. In a word, the former costs are sunk costs, whereas the latter are escapable or avoidable costs. This distinction is most clearly revealed when total revenue requirements are based on an original-cost standard of rate making, as is the case today in most jurisdictions. But it is nonetheless present even when the rate base is measured by "fair value" or by "replacement cost" in any sense of these terms accepted in actual practice by commissions and appellate courts.

In view of this difference between the type of cost that measures revenue requirements and the type of cost that is allocable to the specific services of a utility enterprise, the problem of securing a rational apportionment of the former cost is not essentially different from that which an analyst would face if called upon to apportion among the different groups of customers of a municipal power plant the burden of supplying annual revenues sufficient to yield operating expenses plus enough extra income to finance the construction, say, of a city court house, a menagerie, and an outdoor swimming pool.

On occasion, to be sure, the two above-noted circumstances that tend to defeat any attempt to derive the costs of the different classes of service by an opportionment of total costs—first, the failure of marginal costs to equal average cost and, second, the distinction between sunk cost and avoidable cost—may partly offset each other. During a period of inflation, for example, rates set at long-run marginal or incremental costs, without benefit of any surcharge, might well suffice to cover, or more than cover, a company's entire revenue requirements as long as these requirements are measured by an original-cost standard of rate making. But any close approach to a precise offset could occur only by rare coincidence. And merely for the sake of simple exposition in this brief paper rather than because it makes any essential

difference for the theory of cost analysis, I shall here follow the tradi-
tional assumption that a summation of marginal or incremental costs
would fall substantially short of total costs.

Two Alternative Theories of Full-Cost Apportionment

Assuming, then, that any apportionment of total costs must over-
state the costs allocable on a cost-causation basis, to the different
classes of service, what significance for rate-making purposes can be
attached to such an apportionment? Here two, quite different, answers
are suggested by the practice and comments of the analysts, although
much of this practice and some of the comments appear to be of a
hybrid nature.

The first possible answer is that the apportionment should be de-
signed to reflect, not the absolute but the relative long-run marginal
or incremental costs of the different services. Thus each class of service
might be assigned a portion of the total cost equal, say, to 125 per cent
of its incremental cost. Used as a provisional measure of a sound rate
structure, such an apportionment would have whatever merit can be
claimed for the view that, other things equal, all rates should be made
proportional to incremental costs even if budgetary constraints require
that they be set at a level in excess of these costs.

The alternative answer, which might possibly be accepted by some
professional analysts but which would be sharply rejected by others,
is that the apportionment should represent, not an attempt to measure
even the relative allocable costs of the different classes of service, but
merely an attempt to go as far as formulas can go in assigning to the
different customers responsibility for bearing their fair share of the
total burden of running a utility business. Here would be room for
formulas, such as those used by the Tennessee Valley Authority in its
imputations of joint costs, which indirectly invoke value-of-service or
relative-benefit criteria of rate making—criteria completely foreign to
cost analysis in a strict, behavioral sense.

Suggested Resort to a Two-Step Method of Cost Imputation

If a choice had to be made between the two alternatives just set
forth, the former would seem to me to be much more useful as an aid
to the design of the rate structure. But even this choice would be that
between the horns of a dilemma. And unless the whole procedure of
full-cost apportionment is to be discarded in favor of analyses limited
to estimates of incremental costs, serious consideration might well be
given to an attempt to avoid this dilemma by the acceptance of a third
alternative. This alternative would call for the application to public
utilities in general of a double-step technique of cost imputation some-

what like that used (how effectively I am not prepared to say) by the cost-finding section of the staff of the Interstate Commerce Commission for application to transportation companies. Here, the first step would be to estimate the long-run marginal costs of the different classes of service. The second step would involve the formulaic apportionment, among the same classes, of all unallocable residues of total cost—residues, incidentally, which might be either positive or negative for reasons already suggested.

One of the advantages that might be expected to result from such a change in outlook and in technique would be that of freeing the analyst from the pressure, under which he now labors, to divide all of his costs into groups of cost, each of which is taken to be a function of some single dimension of output. Consider, for example, the analyst's conventional division of the costs of gas and electric utility companies into customer costs, energy or commodity costs, and demand costs. Many, if not most, analysts include among the customer costs not only those meter reading, meter maintenance, and billing expenses that obviously vary fairly directly with number of customers but also a substantial share of the capital and maintenance costs of the distribution system—a share measured sometimes by the estimated cost of a phantom system of minimum capacity but sometimes merely by a graphic intercept. The argument here is that these minimum costs are neither energy related nor demand related; and, so far, this argument is quite sound. But the further assumption is that these same costs are therefore closely related to number of customers. And this assumption is quite unwarranted, since it ignores the factor of customer density. Hence, unless more than three parameters are deemed worth introducing for purposes of rate making, rational cost analysis would put these minimum distribution-network costs into the category of an unallocable total cost instead of forcing them arbitrarily into a catchall category misnamed customer costs. But the cost analyst who adheres to prevailing traditions dare not avail himself of this solution. For he is the prisoner of his own doctrine that he can somehow spread the total costs among the different classes of service so as to report the actual costs of serving each class. He is therefore under an almost irresistible pressure to make his cost imputations seem to support his own views, or those of his associates, as to the proper design of the rate structure. Indeed, as Professor Ralph Kirby Davidson has shown in his stimulating book on *Price Discrimination in Selling Gas and Electricity* (Johns Hopkins Press, 1955), many of the twenty-odd formulas for the allocation of capacity costs cannot be rationalized at all save by reference to some non-cost criteria of reasonable rates.

The Problem of Finding a Rational Method of Apportioning the Unallocable Residues of Total Cost

Even under a double-step method of cost analysis, the analyst would still face the problem of finding some rational or useful formula or formulas for the distribution of the unallocable cost residues. How, then, might this problem be solved?

One possible answer is that these residues should be distributed among the different classes of service in direct proportion to the estimated long-run marginal costs, as a means of designing a provisional rate structure that could be said to be completely nondiscriminatory. Indeed, I believe that the staff of the Interstate Commerce Commission once followed some such practice, although it abandoned the practice at a later date, as was noted by Mr. Ford Edwards in a paper read at the meetings of this Association in 1946 (see *A.E.A. Papers and Proceedings,* May, 1947, pages 441-61). This tentative rate structure would then supply the rate maker with points of departure from which to derive actual rates subject to necessary or desirable degrees of discrimination, the lower limit of these rates being set at marginal cost.

Unfortunately, however, the problem is not so simple, since complete avoidance of those distortionate rate relationships within a given firm which economists associate with discrimination is an unattainable goal, at least as long as any of the rates must deviate from marginal costs. Speaking very generally, the marginal-cost proportionality formula probably comes closer to this goal than would any single alternative formula. But with respect to services that are substitutes for each other in fixed proportions, the principle of rate differentials equal to marginal-cost differentials would supply a closer approach to a nondiscriminatory rate relationship. As illustrations of such services, one may mention coach railroad trips versus Pullman trips between Chicago and St. Louis; or, in the electric power business, industrial power supplied to the consumer at low tension versus an equal amount of power supplied at high tension. Perhaps, therefore, a combination of marginal-cost relationships, some based on cost proportionalities and others based on cost differentials, might be accepted as the most useful basis on which to apportion unallocable total costs as a first approximation of a sound rate structure.

One other question, related to the one just mentioned, concerns the proper allocation or apportionment of a cost which, while a function of some single variable such as maximum demand or output of energy, is a declining function rather than a linear function. Suppose, for example, that if measured on a replacement-cost basis, the total cost of an electric company's generating capacity would amount to $200 per

kilowatt, whereas the incremental cost would come to only $150 per kilowatt. Should the total cost nevertheless be apportioned, for rate-making purposes, just as if it were a linear function of generating capacity? Existing practice seems to support an affirmative answer. But I am not at all sure that the justification for the practice has ever been convincingly spelled out.

Conclusions

I close this paper by expressing the opinion that, in the public utility industries, the most important cost analyses are not those which attempt to apportion total capital and operating costs among the various classes or units of service. Instead, they are the analyses designed to disclose differential or incremental or marginal or avoidable costs—costs which are not derivable from total revenue requirements and which cannot be added together so as to equal this total. In a book on electrical rates published almost forty years ago, Dr. G. P. Watkins wrote as follows: "Were cost accountants willing to (or expected to) deal with less than total expenditures, so far as they claim to obtain the actual cost of a particular good or service, their true service might be more clearly perceived and therefore greater." (*Electrical Rates,* D. Van Nostrand Company, 1921.) Public service commissions and rate specialists might do well to consider the possible wisdom of this statement.

THE EVALUATION OF STATISTICAL COSTING TECHNIQUES AS APPLIED IN THE TRANSPORTATION INDUSTRY*

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I

Statistical techniques to cost transportation activities have been the subject of evolutionary adaptation and controversy in regulatory proceedings ever since their early use in 1923 [1]. This paper perpetuates that tradition by advancing two basic contentions: that very modest adaptations in the conventional costing procedures now used for regulatory decisions would result in greatly improved cost estimates; and that many criticisms of statistical costing procedures are either incorrect or irrelevant, often representing narrow preoccupation with extraneous and unimportant detail. In cost estimation, as in most decision problems, the objective is not to attain perfection but rather to attain the best possible estimates within stipulated time and cost constraints.

As explained elsewhere [2], the use of statistical methods in making cost estimates must be justified on the same grounds as in other scientific endeavor. Basically, statistcial methods are a substitute for experimental controls in attempting to establish and measure causal relationships, statistical techniques being used when controls are either unavailable or too expensive. Unfortunately, the statistical estimate is usually not as precise or intellectually satisfying, especially to the lay observer, as that obtained from a controlled experiment. For this, as well as other reasons, considerably more reliance should have been placed on controlled experiments to determine costs for regulatory proceedings than historically has been the case.

An important corollary of these considerations is that statistical costing is not appropriate when cost relationships can be observed directly. For example, it usually is not difficult to determine, at least to an acceptable approximation, the increase in fuel or labor operating costs attributable to specified increases in transportation outputs. In such instances, where cause and effect relationships can be directly discerned, the estimates obtained by ordinary observation are normally sufficiently accurate as well as less expensive.

The appropriateness of specific statistical costing procedures also depends, at least in part, on the objectives to be served. Historically,

almost all statistical cost finding has been related to regulatory proceedings. The concern usually has been with central tendencies or typical behavior rather than with greatest efficiency or lowest attainable costs. This, of course, constitutes an immediate departure from the precepts conventionally enunciated in economics textbooks, as pointed out in a related context some while ago [3]. The emphasis on regulation also has tended to orient the theory—and the practice—of statistical cost finding in transportation industries toward the estimation of long rather than short-run costs.

Statistical costing should be useful, however, for managerial as well as regulatory decisions. In fact, statistical cost estimates already are employed by a few U.S. railroads for purposes of budget preparation and control. For internal control purposes, short-run and minimum attainable cost estimates are often more useful than the long-run, central tendency cost concepts that have dominated regulatory thinking. In terms of statistical procedures, this also means that a useful analysis for internal control is likely to be based on time-series data for the particular railroad employing the technique. By contrast, cost estimates for regulatory procedures are usually considered to be more appropriately estimated from cross-section data on a large number of railroads.

Similarly, the use of particular cost concepts or costing procedures does not necessarily predetermine policy questions. For example, the fact that a costing technician might consider it inappropriate or impossible to fully allocate certain categories of costs to basic output or service categories does not necessarily imply an endorsement either of a policy of marginal cost pricing or of subsidizing railroad activities. These public policy questions clearly will be resolved differently by persons with different social preference functions and concepts of how technological progress is achieved in a free enterprise economy—to mention only a few considerations. These "subjective considerations," on the other hand, should not enter into the costing procedure. For example, if certain categories of costs do not vary with observable changes in output and cannot be allocated without making arbitrary assumptions, it is the duty of the technician to point out these facts. Not to do so would be a delinquency, though no more so than suggesting that the existence of unallocable costs unequivocally implies a certain set of public policies with respect to pricing and subsidization.

II

The traditional approach to regulatory cost finding has been very much conditioned by accounting procedures and what might be called the accounting viewpoint. Specifically, the emphasis has been upon

dividing a total cost figure into parts that have, hopefully, some functional or operational meaning.

A particularly relevant example is railroad costing. Extremely complex problems have arisen with those rail cost categories that are necessarily influenced by several different kinds of operation; e.g., railroad maintenance of way and structure expense that, for obvious reasons, normally have been considered to be related to both passenger and freight activity. For so-called "joint costs" of this type, the first step usually has been to divide the cost figure into freight-related and passenger-related components. Almost invariably this division has been performed by assuming that the costs are incurred in exactly the same proportions as some common measure of output, usually gross ton-miles. Thus, if passenger business accounted for 40 per cent of the total gross ton-mileage while freight operations represented the remainder, costs would be assumed to be 40 per cent attributable to passenger operations and 60 per cent attributable to freight operations. In short, a gross ton-mile of freight is assumed to have the same impact or causal influence on cost as a gross ton-mile of passenger operation. This equal weighting or neutrality assumption has been widely disputed by engineering evidence and testimony and would seem, even on casual observation, to be rather dubious.

The next step, after this rather arbitrary division between freight- and passenger-related costs, conventionally has been further to divide the freight and passenger totals into fixed and variable components. At this point statistical procedures usually have been brought into regulatory costing. Generally, a line has been fitted by least squares to highly aggregative data (for example, taking total railroad operating costs as the dependent variable and gross ton-miles in all kinds of rail services as the independent variable) to determine the variability of costs over cross-section samples of individual companies. It has been conventional, moreover, to use ratio data in these regressions so that both the cost and output data would be divided by some measure of size; e.g., miles of roadway or track. In graphical form, the situation would be shown in Figure 1. After fitting the line, as shown in Figure 1, an average percentage variable would be computed by considering the point of intersection between the line and the vertical axis as defining fixed cost and letting variable cost be the distance between this intersection level and the line itself at the average output level. Thus, in Figure 1 the average percentage variable would equal V/T. This average percentage variable would then be applied multiplicatively to the previously determined passenger and freight cost estimates to determine average variable passenger costs and average variable freight costs.

Though this is a correct representation of the usual application of statistical methods in regulatory costing, not all applications have been so overly simple in concept. One common elaboration has been to recognize that the cost function is not homogeneous and therefore the percentage variable will differ at different scales of operation. Accordingly, more accurate percentages have been obtained for particular operations by determining the vertical distance between the intercept and the estimated cost function at a particular firm's level of output. An illustrative example is shown in Figure 1 for the below average level of

AN ILLUSTRATION OF ICC COSTING METHODS
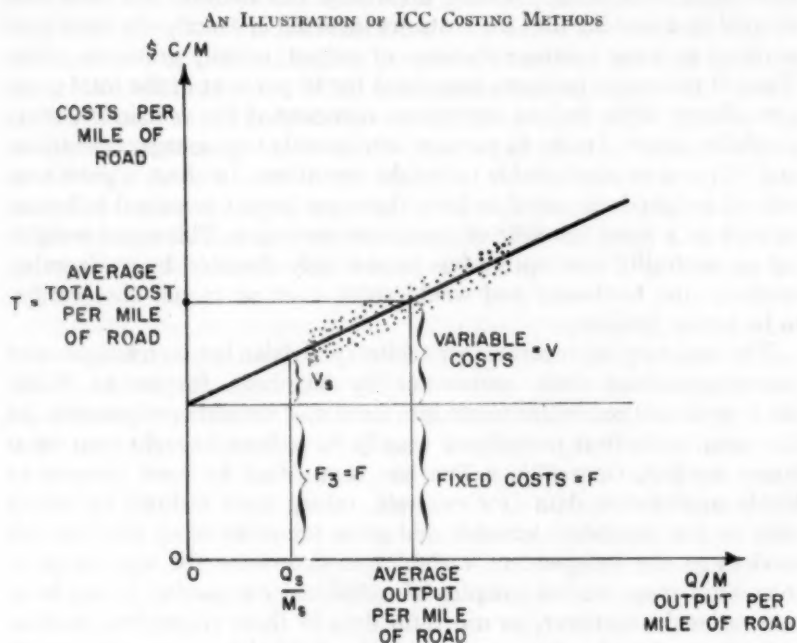
FIGURE 1

output, Q_s. The fixed costs do not decline, of course, with the decrease in scale while the variable costs do. Other important elaborations of the costing procedure have been: the establishment of separate functions for railroads in different regions of the country, though usually no markedly different results have been obtained by this modification; and the use of less aggregative cost categories in the analysis in the hope of improving the quality and accuracy of the estimation procedures.

These are certainly useful modifications but they hardly meet all

objections. Particularly upsetting is the rather obscure fashion in which the statistical techniques have been employed. Basically, the ICC method devolves to estimating a regression function of the following kind:

$$\frac{C}{M} = a + b \frac{Q}{M} + e \tag{1}$$

where C designates cost; Q output of traffic; M miles of roadbed or track (or some other measure of size); a and b are regression parameters to be estimated by least squares; and e is an error or unexplained term. The pertinence of the statistical estimation procedures lies in the estimated values of a and b. Usually, a has been interpreted as an estimate of fixed cost and b as an estimate of incremental or variable cost. If the average value of Q/M is multiplied by b, the result would be the V shown in Figure 1. By elimination, moreover, the average total cost per mile of roadbed must equal C/M at the average output level plus or minus the error term, e. Accordingly, by substitution and ignoring the error term, the ICC computation of the percentage variable can be re-expressed as follows:

$$\frac{V}{T} \times T = \frac{b\left(\frac{\overline{Q}}{M}\right)}{a + b\left(\frac{\overline{Q}}{M}\right)}\left[a + b\left(\frac{\overline{Q}}{M}\right)\right] = b\left(\frac{\overline{Q}}{M}\right) \tag{2}$$

where the bars over the terms indicate that the variable is at average levels. The important point of this exercise is (as might be expected under the linear conditions hypothesized) that the total variable cost at the average output level equals the estimated incremental cost, b, times the average output level ($\overline{Q}/M$). Similarly, if different output levels are considered pertinent to the percentage variable calculation, these output levels should be multiplied by b to obtain estimated total variable cost at that output level.

The estimate of the regression slope, b, is, then, crucial in determining the percentage variable. This, in turn, suggests that attention should be concentrated on this crucial parameter estimate. Such concentration would lead, moreover, to a more purely statistical approach to the costing problem, as contrasted with the hybrid accounting-statistical viewpoint that has dominated traditional methods. A particular advantage of the statistical approach is that it at least permits considering the possibility that different kinds of operations may influence costs differently. This can be achieved, moreover, by very

simple extensions or modifications in the statistical analysis conventionally used in regulatory proceedings.

The essential alteration is to employ multivariate (as contrasted with simple bivariate) regressions. With this change, the questionable neutrality assumption can be tested. In simple geometric terms a multivariate regression means that instead of considering only one quantity or output axis, as in Figure 1, two or more quantity axes are built into the analysis and the fitting problem becomes that of determining a plane with minimum error properties rather than, as in the two-dimensional case, a line with such properties. In algebraic terms, estimating a plane rather than a line means that more complex functions of the following type are to be determined:

$$\frac{C}{M} = a + b_f \frac{Q_f}{M} + b_p \frac{Q_p}{M} + e \tag{3}$$

where the subscripts f and p designate that the output variables now pertain to freight and passenger operations, respectively. The parameter, b_f, is interpreted as the estimate of incremental cost per unit of freight output and the parameter, b_p, as the estimate of incremental cost per unit of passenger output. Thus, while under the neutrality assumption it is assumed that b_f equals b_p, in the more general statistical approach the possibility is entertained that such equality is not sustained by the historical evidence. The interpretation of the intercept, a, is also somewhat altered by the multivariate approach; in a multivariate cost function, a is the fixed common or threshold cost associated with achieving a certain level of efficiency in both freight and passenger operations and no attempt is made to ascribe a to either freight or passenger traffic.

Admittedly, new problems are created by adopting the more general statistical approach. The more important of these are discussed in the next section of the paper. These disadvantages are, however, more than offset by the overriding advantage that the more general approach does not "solve" difficult questions by simply assuming them away. In particular, because of the way in which the historical data may have been generated, especially the lack of controlled or independent variation in the generation process, the statistical analysis may not provide thoroughly satisfactory answers but it will provide the best possible answers within the data restrictions. In short, the statistical approach may not provide entirely satisfactory or, in certain cases, even good answers, but it does make better use of the available information than the traditional accounting-statistical approach and certainly provides less arbitrary cost estimates than the older techniques.

III

Equation 3, while an improvement over the traditional formulations of the costing problem, is still less than fully satisfactory even in a purely statistical sense. Particularly questionable is the weighting scheme built into the statistical estimation procedure through the deflation of the variables by the size variable, M. There is a simple statistical question of what is the most appropriate form in which to include the plant size variable in the analysis.

The statistical appropriateness of different methods of introducing the plant size variable will depend on whether the original data are heteroscedastic; that is, whether the distribution of the dependent cost variable fans out in a wider distribution as the values of the explanatory variables increase. Careful inspection of the relevant empirical data for several railroad cost functions strongly suggests that while heteroscedasticity does exist for the small railroads with under about 3,000 miles of total track, it is not observable for the larger railroads. Thus, weighting the regression or deflating by M might be appropriate for analysis of these small roads but would not be appropriate for analysis of the larger roads. Since most interest resides in the cost behavior of the larger intercity railroads, one obviously appropriate procedure is to divide the sample into two parts: that which is heteroscedastic and that which is homoscedastic; and this division would correspond, respectively, to roads with less than 3,000 miles of track and to roads with over 3,000 miles of track. Separate analyses then could be performed on each set of roads, one involving weighted regressions and one not.

Confining attention to the larger roads, for which no deflation should be performed, the following type of regression function might be fitted:

$$C = aM + b_fQ_f + b_pQ_p + e \qquad (4)$$

which can be obtained from Equation 3 simply by multiplying by the deflator, M. Rewriting the function in this fashion reveals that Equation 3 involves either a confusion or merger of size-related and fixed common costs or the assumption that no fixed common costs exist. Obviously, the latter is a rather limiting assumption and the model described by Equation 4 can be generalized slightly by allowing for the possibility that some fixed common costs exist simply by writing another constant into the function as follows:

$$C = K + aM + b_fQ_f + b_pQ_p + e \qquad (5)$$

where K is now the common fixed cost while a is a measure of the size-related or plant capacity costs. Abandoning the deflation procedure where inappropriate therefore has the additional advantage of per-

mitting a distinction to be made between size-related and fixed common costs through a more precise definition or delineation of the major influences on costs.

An economist, moreover, might challenge the legitimacy of including any sort of size measure, contending that for most regulatory purposes long-run cost estimates would be far more pertinent than short-run estimates. Prompt, short-run adjustments, particularly in rate making, seem to be virtually impossible under regulatory proceedings, making long-run cost estimates normally more pertinent than short-run. Including the size or plant capacity measure in the cost equation tends to yield something more akin to a short-run function, so that a more appropriate procedure might be to eliminate all size variables from the analysis and to fit cost functions of the following form:

$$C = K + b_f' Q_f + b_p' Q_p + e \tag{6}$$

where the regression coefficients explicitly refer to long-run estimates and the primes indicate that the incremental cost estimates were obtained from a cost equation that does not include plant capacity or any other size measure as an explanatory variable. It would be expected that $b_f' > b_f$ and $b_p' > b_p$.

The real question for regulatory procedures is whether the plant capacity included in Equation 5 is really adjustable in a reasonable period of time after an adjustment in output levels may have made an adaptation desirable. For example, it might be contended that the incremental cost estimate from Equation 5 would be more pertinent for assessing the avoidable costs of passenger operations for western railroads while Equation 6 would be more pertinent for eastern roads. For a western road, not only is passenger business less of the total, but also the operation usually is more strung out, has more single-track mainline, and is accordingly less divisible so that a reduction in passenger output might not permit much adjustment in plant. In fact, the correct estimate of *avoidable costs* in most instances probably would lie somewhere between Equation 5 and 6 estimates. The exact compromise would depend on the particular instance. This has led to the presentation of cost estimates obtained by both kinds of functions in previous studies [2, 7, 8]; at a minimum, this dual reporting permits the reader to more or less bracket the range of relevant possibilities and to make his own descision as to what is appropriate.

As already noted, statistical costing involves some new departures in technique but is not a basically revolutionary concept. As in traditional cost accounting, the statistical cost estimate depends, basically, on fitting a functional relationship to historically recorded cost-output experience. For example, the cost accountant often *fits* by simply reducing all the available experience to simple averages, taking, say, total

cost and dividing it by total output to obtain an average cost per unit of output. Geometrically such a procedure reduces to simply determining the cost function by drawing a line through the average cost-average output point and the zero cost-output point. An illustrative example of how this might differ from least-squares functions is shown in Figure 2, where the line marked *A* is the accountant's cost function, determined by the simple averaging method, whereas the line marked *LS* represents the least-squares function.

There is no reason, of course, why attention need be restricted to the Equation 5 and 6 representations of the cost experience. For example, it might be argued that according to economic theory a cost

AN ILLUSTRATION OF DIFFERENT COSTING CONCEPTS
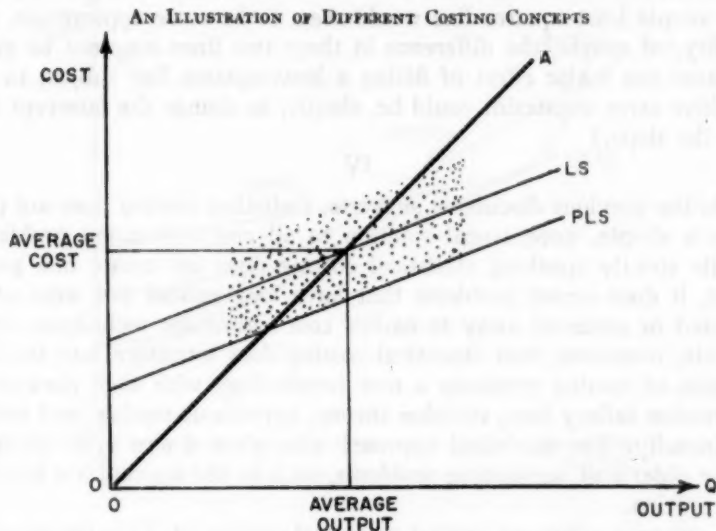
FIGURE 2

function should represent the lowest cost possibilities for a given level of output. The appropriate long-run cost function thus might be an envelope representing the minimum cost points of all short-run cost functions. As can be seen from Figures 1 and 2, the least-squares line tends to go through the middle of the available cost experience. A theoretically more appropriate estimate of the long-run cost function therefore might be obtained by minimizing the sum of squares subject to the constraint that all the error terms have positive signs; a line designating such a positive error function is shown in Figure 2 as *PLS*.

The question of whether the envelope curve or the average experience curve, as determined by straightforward least squares, is the more

appropriate function for regulatory decisions depends mainly on what the regulatory function is considered to be and to what extent there really is a unique envelope curve for the operations under regulation. For example, if only one relevant envelope curve is believed to exist, all deviations from this curve are due to movements along short-run cost functions. Then, if the objective of regulatory policy is to have everyone operate in as nearly an efficient manner as possible, the relevant curve probably would be the envelope or *PLS* line. On the other hand, if it is believed that inherent differences exist in the efficiency and operating situations of different railroads (so that there are also differences in their envelope curves) and that goverment regulatory agencies must take the world as it is, with its efficiencies and inefficiencies, then the simple least-squares line would seem to be more appropriate. (In reality, of course, the difference in these two lines may not be great because the major effect of fitting a least-squares line subject to the positive error constraint could be, simply, to change the intercept and not the slope.)

IV

As the previous discussion suggests, statistical costing does not provide a simple, unequivocal solution to all cost estimation problems. While strictly speaking statistical costing does not create new problems, it does reveal problems that have long existed but were often ignored or assumed away in earlier cost estimation techniques. It is certain, moreover, that statistical costing does introduce into the discussion of costing problems a new terminology with such phrases as regression fallacy bias, decision theory, hypothesis testing, and multicollinearity. The statistical approach also gives a new orientation to many older cost accounting problems, such as the appropriate level of aggregation.

A common argument against statistical costing [4, 5] is the possible existence of so-called "regression fallacy" bias, an objection that applies mainly to attempts to estimate long-run cost functions from cross-section data. Observations on a cross-section sample, it is contended, normally vary by a transient short-run component from a true or long-run equilibrium position and these transient components can be expected to be distributed so that a function fitted to the cross-section data will yield a biased estimate of the long-run relationship that is sought.

An illustrative example is shown in Figure 3. There the long-run total cost function, *LRTC*, has been drawn to the general shape that characterizes most railroad cost relationships. Tangent to this long-run function (which, of course, is an envelope curve) are three short-run total cost functions, *SRTC*'s, that correspond to three different levels of

plant capacity. In addition, three heavy dots have been placed on each of these short-run functions to indicate an actual level of operation at some hypothetical period of time in which all three plants are assumed to be operating at less than full utilization of their optimum or normal capacities. Furthermore, the extent of capacity under utilization is assumed to be proportional for all three firms. Accordingly, if these three points constituted a cross-section sample of observations, a line fitted to them would be like *LS* in Figure 3 and would have a somewhat more substantial slope than the actual long-run total cost function. The

SHORT- AND LONG-RUN COST RELATIONSHIPS
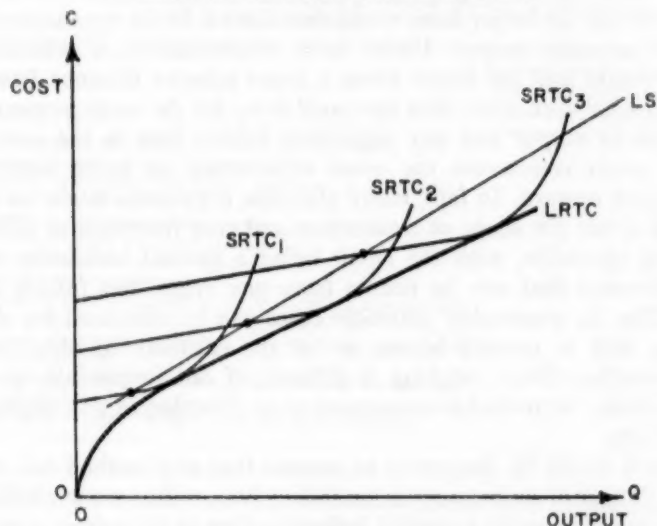
FIGURE 3

extent of any bias will depend on the intrafirm variation in the transient components compared with the interfirm variation of the cost and output experience represented in the sample. For example, in the extreme case of only one size of firm existing in an industry (in which case, of course, a long-run relationship could not be estimated from cross-section data by any technique) all the observable variation in a cross-section sample would be attributable to short-run or transient effects and very poor long-run cost estimates might be expected. On the other hand, for an industry like railroading, where a substantial range between the smallest and largest roads usually exists in a cost estimation sample, the short-run or transient variance usually will be small relative to the interfirm variance.

Obviously, a number of assumptions are required to evaluate the character and presence of regression fallacy bias. For example, it greatly simplifies the evaluation if variations from optimum are shared proportionally. An implicit assumption usually is made, moreover, that approximately the same short-run reaction functions exist for all observations. For example, in cost problems it is particularly important that different sizes of firm cannot or do not build different degrees of flexibility or adaptability into their plants [6]. If larger firms tend to have more adaptability (say, because they have a wider range of choice about the combinations of factor inputs that they can employ for different variations in output around the optimum), the short-run total cost curve for the larger firms would then have a flatter appearance near the full capacity output. Under such circumstances, a reduction in output would pull the larger firms a lesser relative distance from the long-run total cost curve than the small firms for the same proportional reduction in output and any regression fallacy bias in the cost estimation could contravene the usual expectation by being downward rather than upward. In fact, many plausible arguments might be made a priori about the shape of production and cost functions at different scales of operation, with the result being a general ambiguity about the inferences that can be drawn from any regression fallacy argument. That is, reasonably plausible cases can be advanced for downward as well as upward biases, or for the existence of offsetting or other complex effects, making it difficult, if not impossible, to generalize about the probable consequences or direction of any regression fallacy bias.

While it would be dangerous to assume that any method can eliminate all danger of serious regression fallacy bias, means are available for at least minimizing its potential influence. One is to employ a sample that is reasonably rectangular in its distribution [4], an objective which can be achieved to a fair degree of approximation in many cases by simply eliminating the extremes in the sample. Another—and probably more expeditious—method of minimizing regression fallacy bias is simply to use data that have been averaged over several years of experience. This reduces the potential influence of any one extreme year of relative inactivity or overactivity and, furthermore, tends to increase the possibility of offsetting years of underactivity against years of overactivity. For these, among other reasons, some previous railroad costs studies [7, 8] were based on data averaged for a four-year period and with the largest and smallest railroads omitted.

It might be possible, moreover, to test formally for the existence of regression fallacy bias. In fact, one of the present authors now has under way a fairly extensive effort in this regard. Space limitations per-

mit reporting here only the bare rudiments of progress on these studies. One potentially useful technique, however, would be to test for homoscedasticity after fitting a least-square line subject to the constraint that all observed errors are positive; that is, a line like *PLS* shown in Figure 2. If the variance of the residuals for observations on large railroads were significantly different from that for the small, there would be at least presumptive evidence that regression fallacy bias was present. On the other hand, if there were no strong evidence of hetero-

TABLE 1

POSITIVE ERROR VARIANCE RATIOS FOR ELEVEN RAILROAD MAINTENANCE
OF WAY AND STRUCTURE ACCOUNTS

Account	Smallest Five Railroads to Largest Five	Smallest Ten Railroads to Largest Ten
Overhead expense: maintenance of way and structures........	.55	.74
Maintenance of stations and office buildings................	1.01	.98
Maintenance of roadway buildings, telephone and telegraph lines, etc..	1.88	1.13
Maintenance of water and fuel stations.....................	.77	1.09
Maintenance of shops and enginehouses	.74	.87
Removing snow, ice and sand expense......................	.84	1.02
Public improvements maintenance..........................	1.04	.83
Maintenance of yard switching track net of bridges, tunnels, and elevator structures...	.53	.60
Maintenance of way switching track net of bridges, tunnels, and elevated structures.......................................	.94	1.39
Maintenance of running track net of bridges, tunnels, and elevated structures...	.53	.61
Maintenance of bridges, tunnels, and elevated structures......	.77	.91

scedasticity, the results may or may not be subject to regression fallacy bias; that is, the evidence would be inconclusive. In short, sufficient but not necessary conditions for the presence of regression fallacy bias can be established in this way.

A major difficulty with applying this approach at the present time is the lack of a good statistical test for evaluating the significance of differences in error variances generated by the positive least-squares line for different size groups. Not only are the usual normality assumptions underlying these tests badly violated but several other serious difficulties are present. At this stage, therefore, our evidence on this matter is mainly heuristic and definitely deficient in formal probabilistic content.

The evidence, for what it is worth, is summarized in Table 1 for some railroad cost functions previously estimated [7, 8] from averaged data for the years 1951-54. Shown in the table are two sets of positive error variance ratios: those for the smallest five railroads to the largest five and those for the smallest ten to the largest ten for eleven maintenance

of way and structure cost accounts. (Two maintenance of way accounts included in the earlier cost analyses are excluded here because of a scarcity of observations: maintenance of power plants and power transmission systems and dismantling retired road property.) The positive errors were estimated by simply shifting, in parallel, the original least-squares functions downward. A unitary ratio in Table 1 would indicate exact equality of the error variances and homoscedasticity, substantially less than unity would indicate less error disperson for the small firms and the probability of a positive regression fallacy bias, and substantially more than unity would indicate a bias toward underestimation of the true cost coefficient from the simple least-squares fit. As shown in Table 1, most of the variance ratios do not depart too markedly from unity. To the extent that there is regression fallacy bias in these cases, it would appear, on balance, toward overestimation. The over-all extent of any bias would seem, however, to be small, especially when the probable cases of underestimation are balanced against those of overestimation.

Accordingly, the actual or best long-run cost estimate probably lies somewhere between the long-run and the short-run cost functions obtained by first excluding and then including a capital stock variable. Furthermore, these variance ratios lend sustenance to the suspicion expressed earlier [2] that the likelihood is strong that the true long-run marginal cost is closer to the coefficient obtained before rather than after inclusion of the capital stock variable in the cost equation.

In addition to the heuristic test using the positive error variance ratios to determine the existence of any regression fallacy bias, a nonparametric test was used to determine whether the residuals for the small railroads and the residuals for the large railroads belong to the same population. This is an alternative method of testing for regression fallacy bias, related to but somewhat different from the heteroscedasticity test. The test applied was the Mann-Whitney U test, a useful alternative to the parametric t test when it is desirable to avoid the assumptions required for the t test [9].

The U test was performed on two sample sets: Set 1 included the samples consisting of the five smallest railroad systems and the five largest railroad systems; Set 2 included the corresponding samples for ten railroad systems. The results of the tests indicated that for no cost account could the hypothesis that the residuals of the small and large railroads belong to the same population be rejected. This conclusion was reached by using a two-tailed test for a 95 per cent level of significance ($\alpha = .05$). Even in the worst case the null hypothesis could not be rejected at a 42 per cent level of significance.

In general, the use of statistical tests in cases like this must be care-

fully considered. In most statistical costing the assumptions on which the standard parametric tests are constructed are not satisfied. For example, elementary economic theory does not permit the existence of decreasing total costs with increasing output. Hence, the true slope of the cost function with respect to any output variable must be non-negative; the standard statistical tests certainly embody no such assumption. In statistical cost estimation, the standard parametric measures of statistical significance only indicate reliability or confidence very crudely. In circumstances where tests are desirable it may be useful to consider relevant nonparametric statistics. Tests based on such statistics often have surprising power. Unfortunately, many situations arise where no fully adequate test, either parametric or nonparametric, has been developed. In such cases, either a specific test must be developed or some rough guide must be established, such as the positive error variance ratios used in this study as a measure of possible differences existing between large and small railroads.

Another problem in statistical costing is determining the appropriate degree of aggregation. Actually, this problem, in all of its complexity, refinements or lack thereof, has been carried over almost intact from earlier, nonstatistical studies of costs. In railroad regulatory costing the arguments have usually focused on two central questions. First, how detailed a breakdown into underlying or basic accounts is required for a good analysis? Second, are the observable regional differences in railroad costs explained by appropriate disaggregation or do they require special analysis?

From the statistical standpoint, the question of what constitutes an appropriate level of aggregation is basically that of determining the dimensions of the underlying data. In the specific case of railroad costing, for example, the problem reduces to determining the extent to which different rail cost accounts move independently of one another. There is no purpose, of course, in performing separate statistical analyses on conceptually similar cost accounts that also move in virtually complete synchronization; research economy and common sense usually dictate that these accounts be analyzed as a group. About the best-known statistical means of performing a dimension analysis of the type required is by the application of principal components analysis [10]—a technique that was originally developed for psychometric investigations and has thus far been little employed in econometrics. The basic idea of the technique is to construct new variables or indexes that are linear functions of the original variables, are independent of one another, and account for as much of the variance of the original variables as is possible with a minimum number of new or constructed variables.

TABLE 2

A PRINCIPAL COMPONENTS "DIMENSION ANALYSIS" OF THIRTEEN RAILROAD
MAINTENANCE OF WAY AND STRUCTURE COST ACCOUNTS

Variable Number*	FACTOR LOADINGS					
	Component No. 1	Component No. 2	Component No. 3	Component No. 4	Component No. 5	Component No. 6
1	−.94	.19	.03	.09	.05	.00
2	−.95	−.10	−.19	−.06	−.05	−.07
3	−.97	.04	−.09	.06	−.02	−.06
4	−.83	−.31	.12	.34	−.23	.13
5	−.92	−.28	.11	.08	−.20	−.04
6	−.39	−.60	−.64	.02	.19	.18
7	−.41	−.72	.43	−.04	.24	−.18
8	−.79	.32	−.34	.06	.01	−.35
9	−.80	−.24	.04	−.47	−.19	−.03
10	−.82	.30	−.05	−.20	−.00	.19
11	−.88	.26	.24	−.03	.12	.23
12	−.95	.21	.07	.08	−.05	.01
13	−.87	.22	.13	.02	.36	−.01
% of total variance..	68.6	12.7	6.6	3.2	2.9	2.3
Cumulative % of total variance....	68.6	80.3	86.9	90.1	93.0	95.3

* Variable code is as follows:
   1. Overhead expense: maintenance of way and structures
   2. Maintenance of stations and office buildings
   3. Maintenance of roadway buildings, telephone and telegraph lines, etc.
   4. Maintenance of water and fuel stations
   5. Maintenance of shops and enginehouses
   6. Maintenance of power plants and power transmission systems
   7. Removing snow, ice and sand expense
   8. Dismantling retired road property
   9. Public improvements maintenance
  10. Maintenance of yard switching track net of tunnels, bridges and elevated structures
  11. Maintenance of way switching track net of tunnels, bridges and elevated structures
  12. Maintenance of running tracks net of tunnels, bridges and elevated structures
  13. Maintenance of tunnels, bridges and elevated structures

An illustrative example of the application of such a technique is
shown in Table 2 where the application of a principal components
analysis to thirteen maintenance of way and structure cost accounts
(used in an earlier cross-section study [8] of railroad passenger costs
for the years 1951-54) is summarized. The factor loadings, that con-
stitute the heart of the table, are the weights assignable to each of the
thirteen original variables in constructing the new or index variables.

The squared value of each factor loading indicates the percentage of
the total variation of a given variable accounted for by a particular
component; for example, component 1 accounts for approximately 88
per cent of the total variation in cost variable 1. In the same vein, the
bottom two rows of the table indicate the percentage or total variance
in the variables accounted for by each of the new variables (or its
equivalent component) as well as the cumulative percentage of the
total variance accounted for by a particular component and all of the

preceding components. A most significant fact brought out by Table 2 is that six of the newly constructed variables are able to account for a little over 95 per cent of the total variation in the thirteen cost accounts. This immediately suggests that something less than thirteen cost accounts might have been appropriately used in the original study. The loadings provide, moreover, very definite clues on just how the accounts might have been aggregated. For example, looking at the first component, it is reasonably clear that this first and most important of the constructed index variables accounts for a large percentage of the total variation in four variables (Nos. 1, 2, 3, and 12); accordingly, aggregation of these four accounts into one account would seem to be reasonably well justified. At the other extreme, it is clear that variables 6 and 7 (maintenance of power plants and power transmission systems and removing snow, ice and sand) follow very different patterns of variation from the other variables and these two accounts are probably best analyzed separately. Similarly, accounts 8 and 9 also seem to follow somewhat independent courses and, again, might be best analyzed separately. On the other hand, accounts 4, 5, 10, 11, and 13 are somewhat difficult to classify; while somewhat differentiable from accounts 1, 2, 3, and 12, it seems highly probable that they could be analyzed along with these accounts without any serious loss of information.

Sometimes attempts are made to give each of the constructed variables, or components, descriptive titles. As already noted, the first component seems to be so universal in character that it might be described as a general maintenance account factor. The second component is more difficult to classify since most of its variation is accounted for by two rather different variables: maintenance of power plants and power transmission systems and snow, ice and sand removal; about the best descriptive label that might be attached here is the rather bifurcated one of an "electricity-bad weather factor." (It should be noted, incidentally, that in fixing the identity of these factors it is not particularly important to take account of the signs of particular factor loadings so long as the major factor loadings for a particular component are of the same sign; on the other hand, if the major factor loadings or weights have different signs they would obviously have an offsetting effect in estimating a final value for a particular observation and these differences would have to be taken into account in establishing any identity.) Similarly, factors 3, 4, 5, and 6 might be tentatively described, respectively, as: electrification; public improvement; bridge, tunnel and elevated structures; and dismantling retired property factors. For obvious reasons, too much importance should not be attached to these identifications. Their main purpose is to emphasize the identity of those variables that pursue a rather independent course of variation from the

rest. In short, the major points to be derived from the preceding component analysis are: that less than thirteen linear variables apparently are needed to account for most of the variation in the thirteen original accounts; that certain of the accounts (particularly Nos. 6, 7, 8, and 9) seem to require separate treatment; and the separable characteristics in the maintenance of way and structure accounts seem to have a strong regional flavor, being attributable primarily to electrification, snowfall, and the extent of public improvements.

Obviously, if a dimension analysis of this kind is to have maximum usefulness, it should be conducted in the preliminary stages of any costing study; unfortunately, time and computation resources prevented the present authors from performing this type of analysis as a preliminary to their previous costing studies.

This same problem of determining the effective number of dimensions also arises with respect to the independent or explanatory variables used in cost studies. Indeed, in our view, this difficulty, the multicollinearity problem, is the most important single problem encountered in statistical costing. The essence of the trouble is that nature has been unkind and has not performed the type of controlled experiments that the cost investigator ideally would like to have when attempting to differentiate between the different influences acting on costs. That is, too many of the important causal influences are found to have moved together or in relatively close historical synchronization rather than independently. In extreme cases, in fact, collinearity can completely break down the statistical estimation problem, in the sense of making it indeterminate. Actually, extreme collinearity, and not just moderate or slight collinearity, usually is required before really serious problems arise for empirical studies in economics. Some collinearity in historical data is, in fact, almost inevitable in the sense that coefficients of correlation between independent variables exactly or even approximately equal to zero are very improbable. Fortunately, complete independence of the explanatory variables is also not required for the proper fitting of statistical functions.

In most instances, the collinearity problem is closely related to hypothesis specification. In collinear situations the investigator often faces a number of alternative specifications of the causal structure that are equally as logical in an *ex post* sense and will apparently do equally well in explaining the behavior under investigation. In costing, for example, several different specifications of the explanatory output variables may serve equally well in explaining variations in costs because the different measures of output are highly correlated with one another. The usual approach to handling collinearity in economics is to try a number of different specifications, all of which are considered about

equally justifiable on theoretical or conceptual grounds, and to accept that one which seems to provide the best explanation of the behavior under study. This general approach also characterizes the earlier costing work of the present authors. In general, we can say, with at least some defensibility, that our final formulations were at least as statistically good as most alternative formulations, and also reasonably intelligent formulations from a theoretical point of view.

Somewhat more formal justification can be given to the statistical procedures, however, by performing on the explanatory variables dimension analyses of the same kind as those previously reported for the dependent or cost variables. The results of such an analysis for the forty-six explanatory variables originally used in a statistical cost study of railroad maintenance of way and structure accounts (the same thirteen as listed in Table 2) is shown in schematic outline in Table 3. Again, the basic technique was to perform a statistical component analysis. As can be seen from Table 3, 95 per cent of the variation of the forty-six explanatory variables is accounted for by eleven newly constructed index variables.

It is reassuring that the first two components in Table 3 seem to be related to freight and passenger traffic operations, respectively. Indeed, the major point established by the analysis shown in Table 3 is that there does seem to be some independent variation between the freight and passenger operations of major U.S. railroads. If there were no independent variation, only controlled experimentation would provide a means for breaking up the collinearity between passenger and freight traffic measures and a basis on which to establish any sort of separation between passenger and freight cost effects. However, the historical independence between freight and passenger operations is not so great that controlled experimentation still might be worth while.

Another implication to be drawn from the results recorded in Table 3 is that the regional, population, and climatic effects are as pronounced in the explanatory variables as they have already been seen to be in the dependent cost variables. It is not difficult, moreover, to make an intelligent identification of the regional explanatory variables that should be used to explain particular cost accounts; for example, snowfall would be an obvious candidate as an explanatory variable for the snow, ice and sand removal account while the population and urbanization variables probably would be good explanatory variables for the maintenance of public improvements.

In sum, there would seem to be approximately ten potentially relevant dimensions to the explanatory variable structure: (1) general freight operations; (2) LCL freight operations; (3) general passenger activity; (4) commuter passenger operations; (5) electrified opera-

TABLE 3

A Principal Components "Dimension Analysis" of Forty-six Explanatory Variables
Used in a Statistical Costing Study of Railroad Maintenance
of Way and Structure Accounts

Component Number	Per Cent of Total Variance Attributable to Component	Cumulative Per Cent of Total Variance	Important§ Variables†	Suggested Interpretation
1	50.6	50.6	Nos. 1, 2, 3, 4, 5, 9, 15, 16, 20, 22, 23, 30, 32, 38, 41, 45, 46	General with special emphasis on freight
2	12.8	63.4	7, 8, 17, 18, 24, 25, 26, 27, 28, 33, 37	General passenger with special emphasis on commuter operation
3	8.0	71.4	6, 7, 13, 17,* 25,* 33, 34, 43,* 44*	Population-regional effects or technology
4	6.5	77.9	28,* 31, 40, 44	Climatic
5	5.0	82.9	19, 21, 35,* 43	?
6	3.1	86.0	19	Steam operations
7	2.5	88.5	11, 19*	State of technology
8	2.2	90.7	33	Snowfall
9	1.7	92.4	40	Passenger way switching
10	1.4	93.8	6	LCL freight
11	1.2	95.0	7, 11, 12, 19	?

* Indicates a sign reversal for a particular variable from the dominant pattern for the components.

† The variable code is as follows:
1. Total expense maintenance of way and structure less retirements and depreciation
2. Number of workers maintenance of way and structure
3. Total expense maintenance of way and structure minus overhead expense maintenance of way and structure
4. Mail, express, and milk revenues
5. Mail, express, and baggage car miles (freight and passenger)
6. LCL freight originated tons
7. Total passengers carried
8. Revenue passenger miles
9. Investment in stations and office buildings
10. Miles of road "other" signal system operation
11. Miles of road CTC operated
12. Miles of road block operated
13. Number of interlocked switches
14. Investment in signals and interlockers
15. Investment in communication systems
16. Investment in water and fuel stations
17. Other locomotive miles road service freight
18. Other locomotive miles road service passenger
19. Steam locomotive miles freight and passenger
20. Diesel electric locomotive miles freight and passenger
21. Investment in shops and enginehouses
22. Gross ton-miles freight
23. Gross ton-miles passenger
24. Investment in power plants and power transmission systems
25. Miles of electric road operated
26. Miles of electric track operated
27. Other locomotive mile freight and passenger road service
28. Average January temperature
29. Investment in snowsheds, fences, and signs
30. Miles of road operated

tions; (6) steam operations (although this is probably more of histori-
cal than current interest); (7) a general climatic or weather factor with
particular emphasis on snowfall; (8) an urbanization-population char-
acteristic; (9) some measure of the state of technology, with particular
emphasis on automatic switching and central traffic control; and (10) a
general size or total investment measure, though this variable was re-
markably ill-defined in the dimension analyses. The component analy-
ses also contain suggestions about which variables might be appropriate
measures for each of these activities or types of operation; for example,
general freight activity bears about the same relationship to a number
of freight output measures, like gross ton-miles or locomotive miles in
freight service, and any one of these variables probably would perform
quite adequately as a measure of the variation for the whole cluster.
The dimension analyses therefore provide additional insight into the
clustering of different railroad activities and also provide evidence in
confirmation of the basic underlying hypothesis of the statistical costing
approach that sufficient independent variation does exist between differ-
ent classes of railroad activities to enable at least some identifiable
separation of the effects of these activities on railroad costs. The neutral-
ity assumption is, in short, both unduly pessimistic in an empirical
sense as well as conceptually objectionable.

V

In the final analysis, the value of statistical costing lies in its ability
to provide answers or guidelines to specific questions. For example, the
objective of the earlier study [8], used for illustrative purposes here,
was the determination of the avoidable costs of passenger train service.
The intention was to determine typical cost behavior of American rail-
roads for regulatory decision making and least-squares estimates were

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31. Snowfall dummy variable
32. Cost of reproducing road new
33. Urbanization
34. Population density
35. Number of crossings
36. Yard switching locomotive hours freight
37. Yard switching locomotive hours passenger
38. Miles of yard track operated
39. Locomotive miles train switching freight
40. Locomotive miles train switching passenger
41. Miles of way switching track operated
42. Average speed×average weight of passenger trains
42. Average speed×average weight of freight trains
44. Temperature range January–July
45. Miles of running track operated
46. Investment in tunnels, bridges, and elevated structures

§ "Important variables" are those that account for approximately one-third or more of the
variation in the specified component or have 80 per cent or more of their total variation ac-
counted for by the particular component.

used. From a decision theory point of view this procedure implicitly assumed that the costs of a given size of error are equal and follow a quadratic pattern for both over- and underestimates. Other criteria may be preferable depending on the purposes for which the estimates are being provided. For example, it might be desirable in some rate-making situations to estimate costs so that firms will not make excessive returns on investment or, alternatively, so that some proportion of the firms makes a minimum of some prespecified return. In such cases, an explicit error cost function should be provided and there may be no special merit in the assumption of minimum least squares. Many cases where minimum least squares is properly applied as the criterion can be better estimated, moreover, by specially designed experiments. It is mainly in those cases where specific policy questions must be answered subject to limited time and funds for research that statistical costing techniques fulfill a need.

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H. Thomas Koplin: What is the function of the economist concerned with problems of government regulation of business? The implicit assumption in both papers is, I think, that it is the evaluation of policy in terms of economic efficiency (in the Paretian sense). Professor Bonbright is concerned that costs be allocated in such a way that rates based on the costs allocated to a product will induce an efficient supply of the product and an efficient distribution of that supply. He grapples with some accounting and regulatory practices which cause a consistent bias away from such an efficiency-inducing allocation of costs, and recommends that attempts to allocate total costs be abandoned. Instead, he would determine the long-run marginal cost for each product (note that this does not constitute cost allocation); to the extent that rates based on these marginal costs do not provide revenues equal to total costs, he would adjust rates, the method of adjustment varying with individual cases. As he points out, there is no single formula for the allocation of these non-marginal costs which will always satisfy the requirements of efficiency.

The paper of Meyer and Kraft nicely complements the conceptual approach of Bonbright with a statistical study which is essentially an attempt to identify the marginal costs in transportation, on the basis of which the regulators (and firms) can use their powers of traffic allocation, direct and indirect, to encourage the efficient use of transportation resources.

The authors of both papers do well the job they have set themselves. The task of defining and identifying efficiency—and inefficiency—is not simple. It is difficult to explain the concepts to students, to businessmen, and to regulators. For the student, economic maturity can be said to arrive when he masters the principle of efficiency. (Sometimes the student never achieves his "moment of truth" and goes through life supported by some rules which substitute for comprehension.) The businessman is usually at home with the concept of efficiency only with respect to money costs within his own business. Historically economists have tended to ignore this important area. Economic theory often simply assumes the individual businessman will find and use the lowest cost method of production. This may have been a reasonable, if not realistic, approach for small-scale business. But with large firms the minimum cost is difficult to find. Here statistical techniques are useful and, as Meyer and Kraft point out, many businessmen have preceded economists in developing the techniques and data. But while the good businessman is familiar with problems of internal efficiency, he is typically poor at accepting, or even understanding, efficiency applied externally. This is unfortunate, but understandable.

But the greatest frustration for the economist concerned with promoting the cause of economic efficiency is created by the regulators, who persistently seem unable or unwilling to perceive economic truths and base their regulatory policy on them.

Why are they so persistent in their apparent economic illiteracy? I suggest we need to consider the possibility that they are more right than we, that they

have a broader and more complete conception of policy criteria than we have, and that we should add to our model of an efficient economy a serious and systematic study of why regulators (and others) fail to proceed expeditiously to accept and implement our model.

To do so means moving beyond efficiency. I am not suggesting we all become psychologists. What I am suggesting is that there may be good economic reasons for the failure of the regulators to accept our model.

What is economics? Is it efficiency? I think not. Some twenty years ago, in his chapter on "Public Utilities" in *Government and Economic Life*, Ben Lewis proposed a threefold test of regulatory policy: economics, equity, and expediency. Let me propose an alternative triad: efficiency, equity, and environment. By equating economics with efficiency, Lewis gave aid and comfort to those who would make technicians of us. Efficiency is a large part of economics, but it is far from the whole. Let us boldly, forthrightly agree that policy should be based on the subjective factors of equity and what I call environment, as well as on efficiency.

Equity considerations are well known to economists. Environment can be roughly defined as the political and social processes and institutions through which efficiency and equity are sought. Equity and environment factors are rarely absent from economists' policy discussions, but they are there on sufferance, on an *ad hoc* basis, often in footnotes or asides. I believe they must be explicitly and systematically incorporated in policy discussions.

It is true that equity and environment are subjective factors, that they rest on personal values, and that the economist has no claim to superior values. But it is equally true that both are as important in determining policy, and human satisfactions, as is efficiency. It is therefore not only proper but essential that the economist incorporate them in his policy analyses. In doing so, he will simply be catching up with the regulators, not to mention the public.

We have been misled by the apparent contrast between the objectivity of the efficiency concept and the subjectivity of equity and environment. While conceptually efficiency is reasonably objective, in application to specific policy questions it is often extremely difficult, if not impossible, to identify or determine. For example, we may agree that efficiency requires rates not less than marginal cost. But these papers are complementary in demonstrating the weakness of the concept of marginal cost in practice—Bonbright mentioning the conceptual difficulties and Meyer and Kraft demonstrating persuasively the difficulties of locating that elusive concept statistically. There are all kinds of marginal cost, and the chances of identifying and measuring accurately the correct one in any particular case are trivially small.

It is often argued that the subjective nature of equity and environment makes systematic treatment of these factors impossible. This is not only an untenable counsel of perfection but also confuses the study of values with the determination of values. The fact that the economist cannot and should not determine values does not mean he cannot provide an extremely useful service in defining, identifying, and discussing values. There is really a good deal to say about equity and environment, in general and in specific cases. For a beginning, we can try to identify the various concepts found in public economic behavior and in regulatory policy and pronouncements. We can study their re-

lationships with each other and with the concept of efficiency. We can search for the implicit assumptions on which they are based. We can compare and contrast the assumptions and their implications. Above all, we can study their relationships—analytically if possible, but at least descriptively—to the satisfaction of human desires, always remembering that happiness depends not only on the goods we have but also on how we received them, and how much others have.

Our goal, to borrow words from Meyer and Kraft, is not perfection but the best possible solution to regulatory problems. And in the theory of the second —or thirty-second—best, we must join considerations of equity and environment to the traditional efficiency.

A systematic, professional approach to equity and environment, plus the economist's traditional competence in dealing with efficiency, will make the economist a stronger guide to regulators. His advice on regulatory policy will be not only more persuasive but also more defensible in terms of the fundamental goal of increasing human satisfactions.

DANIEL MARX, JR.: Niels Bohr relates how physicists at his institute in Copenhagen, when in trouble, used to comfort themselves with jokes, among them the old one of the two kinds of truth. To the first category of truth belong statements so simple and clear that the opposite assertion obviously could not be defended. To the second category, the so-called "deep truths," belong statements in which the opposite also contains deep truth. Unfortunately much of the controversy over full cost versus marginal cost pricing has the odor, at least to my nostrils, of deep truth. But lest pungency be mistaken for cogency, I hasten to add that the papers above represent efforts to escape, insofar as the subject permits, from the deep variety towards simple and clear truth.

The authors of both papers agree that traditional cost accounting methods leave much to be desired in the calculation of specifically allocable expenses in the public utility and railroad industries, respectively, and that the economist can assist in improving costing procedures. There is further agreement that true joint costs can, economically speaking, only be allocated arbitrarily. Professor Bonbright favors a double-step technique of cost imputation for utilities of a type similar to that used by the cost-finding section of the ICC, while Messrs. Meyer and Kraft recommend for railroads the employment of statistical costing techniques, largely of their own development, as substantial improvements over those employed by that regulatory agency. Meyer and Kraft contend, and I agree, that improved statistical accounting can allocate without arbitrariness, or with less arbitrariness, some significant costs hitherto treated as joint.

We are indebted to Meyer and Kraft, not only for their presentation of imaginative costing techniques, but also for a fair and candid statement of the pertinent shortcomings of their application of probability analysis. Also to be commended is their strong recommendation that individual railroads use improved statistical cost procedures and that empirical testing be employed to check on and sharpen their statistical analysis. Nevertheless, despite several caveats regarding the extent of the area remaining wide open for value judgments and the acknowledgment that the assumption of no spurious correlation

represents a basic limitation of their method, which may prove particularly harmful for policy decisions involving structural changes, I seem to sense a note of impatience to employ their findings for the determination of policy matters. To the extent that their results are improvements over previous cost studies, this eagerness may not be wholly amiss; but let us not forget that true joint costs must still be arbitrarily allocated and that their magnitude will in many cases, such as a back haul for example, remain significant.

Conceding at the start the danger of analogies, I am tempted nevertheless to call attention to a suggestive resemblance between the problem vexing this session and the wave-corpuscle duality that for a time perplexed physicists. They were forced to the conclusion that an essential element of ambiguity is involved in the dilemma regarding the corpuscular and wave properties of electrons and photons which provide contrasting pictures each referring to an essential aspect of empirical evidence. The evidence thus obtained cannot, therefore, be comprehended within a single picture, but must be regarded as complementary in the sense that only the totality of the phenomena exhausts the possible information about the objects.

At the risk of reducing the newly sacred to the old and banal profane, this suggests to me Marshall's scissors example of the cutting intersection of supply and demand curves. Therefore we should remind ourselves that even if these papers help us to determine cost schedules with greater accuracy and less arbitrariness, they do not include the complementary picture of demand. Indeed, this raises the additional question of whether electric, water, and railroad freight operations each produce a single product or multiple products. Are we justified in considering the output of each of these industries as homogeneous without more knowledge of the cross-elasticities of demand?

It may seem ungrateful to suggest the study of a complementary aspect to those who have made a contribution to the apposite feature of a knotty problem, but the ingenuity they have displayed in analyzing costs is testimony to their qualifications to study demand. The measurement of demand is exceedingly difficult, but estimates should be made even though the resulting ranges may be subject to still wider margins of error than apply to estimates of long-run marginal costs. To help determine the socially desirable scale of operations, it is necessary to know the shape of aggregate demand, and, where significant joint costs remain, it is also important to know whether single or multiple products are being produced. These remarks should not, however, be construed as a defense of rate discrimination or differentiation as currently practiced.

Many years ago, Yale's former president and economist, Arthur Twining Hadley, is reported to have said that "God Almighty did not know the cost of carrying a hundred pounds of freight from Boston to New York." New Haven scholars were not prone to underestimate the Almighty, but they have been known to underrate gentlemen from Cambridge. Though Hadley's confidence in the inscrutable nature of transport costs will be sustained as long as true joint costs exist, our margin of ignorance, thanks to the gentlemen from Cambridge, is being narrowed. May I again express the hope that they will add, to their already massive effort, equal consideration to the measurement of demand.

RICHARD A. TYBOUT: The two principal papers have been concerned with improved costing for rate structures—and properly within the spirit of the

times. The determination of inherent advantage and the scope for competition among the agencies of transport call for more attention to costs by components and for given outputs.

In particular, multiple correlations permit the identification of changes in common costs with changes in selected outputs. With such information at hand, it is possible to impute a part of every change in common costs to the corresponding output changes. The common costs may be strictly contemporaneous as in administrative expenses, postponable as in maintenance of way, or long-run common costs as in most capital expenditures. Statistical costing determines, within the technical limits of the situation, what parts of common costs are being incurred by specific outputs over the period in question. Although the costs are imputed, they are marginal or variable in the same sense as directly observed costs.

It is a separate matter whether statistical analysis should be concerned with short- or with long-run phenomena; i.e., with time series or with cross-section data. Meyer and Kraft emphasize the importance of long-run data for regulatory purposes. Regulation can properly take the long view. But if we are also concerned with the question of whether competition can play a greater role in the control of railroad rates, it is worth analyzing the speed of adjustment from short- to long-run marginal costs. In a dynamic world, adjustment is a continuing process. And minimum rate controls are very much a part of today's regulatory structure.

The most difficult conceptual problems, of course, face the economist when operations are conducted in the stage of long-run increasing returns. In this case, it is not possible to allocate full costs. Meyer and Kraft refer to the problem as a lack of homogeneity in the cost functions. When dealing with linear relationships over the entire range of outputs, as they are, this must be taken to mean that there are thresholds, or start-up, costs involved. For the sake of grasping the point at hand, we can, therefore, understand the impossibility of imputation as arising from the fact that some costs are not variable at all with output and hence can hardly be correlated with particular outputs.

Parenthetically, we note an anomaly resulting from the use of the same linear relationships over the entire range of output: As long as we assume the same linear relationships to hold for all outputs and in the presence of positive thresholds, it is not possible to make full cost imputations. Referring to the economic theory of the firm, one might conclude that we are always in the stage of increasing returns. This may or may not be the case. Whether we learn to distinguish the stages of production depends on our statistical ability to derive more exact cost curves. The most obvious first step in this direction would be to try to determine a series of piecewise linear relationships that would approximate the true cost curve. Each piece would be a linear relationship over a limited range of output. A linear approximation in the range of decreasing returns, if it were found to exist, could result in full cost imputation for the familiar reasons given in the theory of the firm. This is undoubtedly most consistent with Professor Bonbright's objectives.

In what circumstances can we safely make piecewise linear refinements? The answer depends on the precision of the data, conceptually, and as a matter of observation. Conceptually, there will be increased variance in the data if long-run cost curves are irregular, due, for example, to large indivisibilities in costs

occurring at random throughout the range of outputs. Unexplained variance will be increased by differences in cost due to regional or traffic differences, assuming, again, that there is no systematic relationship between railroad size and region or traffic. If there is such a relationship, of course, there will be the further problem of treating the data in such a way as to prevent statistical bias. Finally, there is the problem of errors in observation, which we shall assume to be random. All of these affect the unexplained variance of the line of regression.

The basic difficulty is that the larger the variance, the less able we are to say whether piecewise linear relationships would be an improvement over a single straight line. It is dangerous to fit new lines of regression over a short range unless variances over all the resulting piecewise approximations are considerably reduced in the process and the result conforms to theoretical expectations. Thus, the statistical appearance of increasing returns may not mean the actual existence of increasing returns, but only a statistical inability to represent the facts any more closely.

By similar reasoning, if variances are large everywhere, it is more difficult to carry out other tests. Meyer and Kraft discuss the problem of estimating regression fallacy bias and conduct a test using variance ratios. Other things equal, the larger the homoscedastic variance, the less helpful the ratio test is.

Perhaps the most intriguing of the many interesting suggestions made by Meyer and Kraft is the use of principal components analysis in the aggregation and multicollinearity problems. It is probably worth noting that similar problems of selection among rival explanatory variables are found in highway costing of both capital consumption and maintenance. The array of competing explanatory variables there includes gross ton-miles, axle-miles, and space-occupancy, to name a few. The extent to which it will be desirable to use statistical analyses will depend on the nature of the data soon to be released from the recently completed AASHO Road Tests.[1]

A final question is raised in both of the principal papers: By what logic can we impute thresholds and other costs not subject to statistical treatment? There is no general answer. Welfare economics provides no guidance on this point, except that we can remain on a Pareto efficient locus by financing the difference with lump-sum transfers. The question of *what* transfers can be resolved only in terms of social preferences and goals, which are not likely to be the same among different policy-makers.

Mathematical techniques are sparking the acquisition of new knowledge over a wide frontier in economics and the social sciences. It is entirely appropriate that these techniques be adapted to transportation and public utilities, not merely because of the well-established data collection systems for regulated industries, but, more importantly, because of the intrinsic interest and general significance of the problems of relating public policy and private management decisions.

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[1] The initials "AASHO" stand for the American Association of State Highway Officials. The road tests have been conducted over the past three years on behalf of members of the Association, the U.S. Bureau of Public Roads, and the Highway Research Board.

WHEAT: A PERMANENT NEED FOR
A FARM PROGRAM?

WHEAT AND FARM POLICY

By JOHN A. SCHNITTKER
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All major farm organizations in the U.S. and both political parties are converging on either reluctant acceptance or outright approval of a policy of running commercial agriculture, like steel, at less than capacity in the sixties; that is, at rates of aggregate output lower than producers would wish to reach at prices determined by public policy. This is the significant feature of the agricultural policy debate today. Agreement on this score is far advanced, though far from complete, but is obscured by continued disagreement over the means to be used in managing or planning farm output and marketings, and by uncertainty over the length of time for which such farm output policies may be needed to meet certain farm price and income objectives. It is marred, also, by dispute over the appropriate price level for farm products in a world where few prices are made any longer the way farm prices were made prior to the adoption of recent farm programs. And it is further obscured by the elaborate rituals of dissent, including the occasional tendency for the peak intensity of a political storm to come just before the calm, not the calm before the storm.

This emerging spirit of general agreement is underscored by several recent events. A small but significant element of farm output and farm price-level planning was explicit in the resolutions adopted in December, 1960, by the conservative American Farm Bureau Federation. The Republican Party in the recent election also leaned heavily on large-scale land retirement as a price supporting device. Both advocates hoped that the program could be temporary—that there was a day somewhere ahead in which American farmers could have good prices and incomes, less government intervention, and no surpluses, and consumers could have reduced food prices at the same time. The stated flexibility of means and the potentially moderate objectives in the farm policy position of the incoming Democratic Administration add to the possibilities for farm policy compromise acceptable to farmers, consumers, and taxpayers.

It was difficult to see this consensus through the smoke of the recent election campaign. But it is sobering both to the winners and the losers

in a political contest to remember that neither unfriendly characterizations of the potential program of the opposition nor unspecific pledges to achieve certain results are the program itself. Those politicians and economists who either invented or took seriously the report that food prices would have to rise 25 per cent and that armies of bureaucrats would necessarily be turned loose on the countryside if the Democratic farm program were adopted, will now have an opportunity to reassess their misgivings on the basis of definitive proposals, not caricature. And those who made and supported the tentative proposals of the other side have the sobering responsibility to decide what they wish their real farm policies and programs to be.

This emerging consensus on a policy to plan the aggregate farm output of the future has its roots in two significant and related bits of economic analysis. The first is the recent work clarifying the concept of the economic structure of agriculture and its relation to other major sectors of the U.S. economy. The second is increasing recognition by economists and the public that these differences in economic organization between agriculture and other sectors of economy lead to divergent results in terms of output, prices, and incomes. Once these matters are recognized, it becomes less attractive to advocate a return to pure competition, including the inability to plan the aggregate supply of farm products, as the ideal form of economic organization for agriculture. To advocate such a form of organization with its apparent results for agriculture but not to advocate its return in other sectors of the economy is, at best, inconsistent.

Professor Clodius discussed the first of these two elements of economic analysis—the comparative economic structure of agriculture and the rest of the economy—at these sessions a year ago:[1]

. . . What the structural factors "control" is the behavior of the firms in the market. Turned around, the theory says that the conduct of firms in any market can be explained by consideration of the structural attributes of the market. The final phase of analysis relates to the performance of the market as a whole which is the aggregate of firms' behavior with the interactions among firms taken into account.

Taken as a whole the theory seeks to explain the performance of an industry in terms of the conduct of the firms in the industry which are controlled by the structural features of the market. Not all the steps are necessary. Sometimes sufficient explanation of performance can be achieved through consideration of structure alone. However, the ultimate explanation involves all three considerations—structure, conduct of firms, and industry performance. Causation is generally assumed to flow from structure to conduct to performance. This does not prevent consideration of feedback in certain markets where the conduct of firms is such as to maintain or to alter the elements of structure.

There are three major features in the concept of market structure. They are the number and size distribution of firms, the class of product, and mobility of firms in and out of the industry.

The outstanding feature of economic structure in U.S. agriculture is the number and size distribution of firms.

[1] Robert L. Clodius, "Market Structure, Economic Power, and Agricultural Policy: A Proposal for Forward Production Control," *J. of Farm Econ.*, May, 1960.

The significant factor of economic conduct which follows from this feature of economic structure is the tendency for farmers to make innovation and production decisions in isolation, as though the aggregate results—those over which they have no control—did not affect them. This feature, coupled with rapid and available technological improvements, results today in chronically excessive farm output.

The result—which Clodius called performance—is the "unplanned character of aggregate supply" of many farm products. Coupled with the demand elasticities of farm commodities, unplanned total output leads to substantial price and income instability and to the prospect of comparatively low returns as a chronic condition.

In contrast to this observed performance of major sectors of agriculture, performance in many sectors of the economy is characterized by successful efforts to account for interdependence among firms and by gearing supply to demand at or near prices which were intermediate objectives of policy consciously arrived at. This gearing supply to demand, at prices which could not otherwise be maintained but which would be extremely profitable if they could be maintained in the face of greater output, is a feature of performance which now merits widespread approval in the most conservative quarters if it can be done unostentatiously and privately either in industry or in agriculture. In the oil industry, under the terms of a publicly-administered interstate oil compact, it is the difference between order and chaos, some say. In scattered sectors of agriculture where supply can be geared to demand at what become accepted as reasonable prices, through group action or with minimum government participation, it is looked upon as wise policy. To fail to take advantage of such an opportunity to guide performance —to plan for profit—is viewed as bad management.

It is in the commodity sectors of agriculture where the structural characteristic of large numbers of firms now appears to preclude such private or small group control of output and prices that the management of aggregate output seems to be regarded with suspicion. But this is only the superficial aspect, for, as noted at the start, conscious output and price policies for agriculture are becoming increasingly respectable, if not universally acceptable. So are those economists—it is hoped—who discuss such policies as means toward certain objectives. It is means, much more than objectives, which are the heart of the current farm policy debate. Galbraith was right when he wrote in 1954:[2]

> On two of the most important subjects of controversy in our time, labor and farm policy, the real issue has been resolved. A struggle, which once involved a great change in the power relationships in the American economy has subsided into a skirmish over the terms of the ultimate accommodation.

Means are important, we can all agree. Substantial and potentially

[2] *Economics and the Art of Controversy* (Rutgers Univ. Press, 1955).

long-term involvement by the federal government appears to be the distinguishing mark of those instruments of output policy which are unacceptable to some. Yet the government would be no less deeply nor any less permanently involved—financially and administratively—in a large-scale land use planning program if it were the chief instrument of supply management, than in a program to limit acreage or marketings of some commodities. So it is not the fact of government involvement but the form it takes which becomes the basis for objecting to certain means of implementing output and price policy for U.S. agriculture. When this is clear, it is clear also that despite the ardor of those who dispute between one means and another, the disputants have much in common.

Other important areas of consensus are implicit in the foregoing discussion.

1. The average level of farm prices in the absence of enabling legislation (permitting marketing agreements and orders, for example) and direct government intervention in farm price determination in the sixties would be substantially below the price level of the late fifties if research and discovery were to continue at a fairly rapid pace and credit were to be readily available. The average income level of commercial farmers would be reduced sharply as the decline in the number of farmers, even with the increased pressure to change occupations, would be too slow in a decade or less to offset reduced margins per unit, as slightly increased disappearance of products with inelastic demands failed to offset price declines, and as the prices of other goods would continue to rise.

Free market farm prices averaging as much as one-fourth below the late fifties are, in fact, the expected norm for the sixties even if the rest of the economy is exceptionally prosperous and slightly inflationary. It is no longer very controversial to argue that this would be the case both for the short run of 1960-63 and the longer run of 1960-70. A few farm leaders and an occasional economist can be found to state a contrary opinion: that price supports hold farm prices down, not up. I believe the evidence runs strongly against them.

2. It is increasingly apparent that such a price level for farm products would not be tolerable to many of the people who man commercial agriculture today—faced with a threat to economic survival—and to the whole society, which would have the task of absorbing even more farm people into a nonfarm work force with already excessive unemployment.

Man need not submit equally to the benevolence and the tyranny of economic history. The essence of democratic society is that it is a product of reason and moral strength, not submission to circumstances. He has devised pricing institutions—for labor, for industry, for public utili-

ties—with consequences more acceptable than with pricing under pure competition.

3. So a third area of general agreement follows from the first two: an active role by government—that is, by political society—in economic affairs including agriculture in the sixties is not only legitimate but also necessary if we are to program changes in key institutions at rates we wish to accept and at costs we choose to pay.

To understand this bipartisan commitment, we must understand the true aim of political society. It is, in my opinion, well expressed in the words of Maritain:[3]

> . . . to better the conditions of human life itself, or to procure the common good of the multitude, in such a manner that each concrete person, not only in a privileged class but throughout the whole mass, may truly reach that measure of independence which is proper to civilized life and which is ensured alike by the economic guarantees of work and property, political rights, civil virtues, and the cultivation of the mind.

In this view of democratic life, the state is the servant of man, not man the slave of the state or of the inherited establishment. This is, I believe, the dominant American view.

There is some trace of Utopia—of a naïve faith in human and institutional perfectibility—in our reliance on government to alter circumstances which appear to be no longer tolerable to the majority. But the alternative to Utopianism is not despair. Even though change does not always turn out to be progress, man keeps trying to improve the mutual adaptation of environment and self. It is in that spirit that we approach the task of improving the conditions under which farm products are priced and the owners of farm resources compensated.

It is scarcely appropriate to encourage collective bargaining for labor but to decline to search for similar innovations in farm markets. It would be inconsistent to adopt or accept output and employment policies and price and wage policies for much of industry and labor but to reject similar price and output policies (if they can be devised) for agricultural producers. Is it a contribution to economic order and stability to accept an economic structure for agriculture with distinctly different characteristics than the dominant economic structure, and results, if not unique, at least unusual?

Obviously, a certain freedom may be lost in the pursuit of stability and justice and other freedoms. But it is important to distinguish between democratic and totalitarian restraints upon individual freedom. There is little evidence that the modest limits upon farmers' actions to date have been very unfavorably received. Witness the continued popularity of milk marketing orders, the tobacco program, and wheat marketing quotas.

4. We can agree, I believe, that even though the price system is not

[3] *Man and the State* (Univ. of Chicago Press, 1951), p. 54.

all-powerful, it is a useful ally in agricultural resource allocation. Although aggregate farm output responds sluggishly at best to lower prices, it responds considerably more quickly to higher prices. Higher prices for many farm commodities in the early sixties are, therefore, contradictory to our resource allocation objectives. Failure to lower farm prices substantially is not a comparable contradiction, however, since the lower prices would be expected to have a negligible effect on farm production. Substantially higher farm prices would also conflict with the desire to increase exports, to reduce federal budget expenditures, and to hold the line on the cost of living. They would make direct output restriction slightly more difficult and output limitations through land retirement more costly. Even so, if the public were in a mood to accept slightly higher farm prices as passively as it accepts other "uneconomic" price increases, the case against some modestly higher farm price supports and prices a few years from now is not a strong one.

Wheat Programs in the Sixties

Public patience with the federal wheat program may not be exhausted in 1961, but it is not inexhaustible. In view of the expected July, 1960, carryover of 1.5 billion bushels, the projected increase of 150 million bushels from the crop now planted, and the prospect that carryover will rise indefinitely under the present program, early revision appears certain. The question of how to revise it is intimately linked to the matter of who will gain and who will lose. For the wheat program, like full employment, is linked to the hopes and fears of a great many people and to the prospects for growth or decline of economic activity in major sections of the country. We are dealing with more than a federal budget and a glutted granary when we talk of a new wheat program.

Our general topic is, "Wheat: A Permanent Need for a Farm Program?" When we speak of a need for a farm program, I presume we mean that to meet certain objectives—chiefly with respect to maintaining the level and stability of farm income, improving the economic organization and stability of American farmers, providing a stable supply of food raw materials for domestic use, and meeting our world obligations without threatening other agricultural exporters—continuation of some specific federal farm program for wheat is needed. The alternative would be, I assume, termination of the price support and production control programs, not only for wheat, but, in order to be consistent, for all of U.S. agriculture within a fairly short time.

These objectives cited are not wholly consistent with each other, but they are also not totally exclusive. I do not claim to know the "right" ordering of the objectives, but I suspect that just as the welfare of the American worker is an extremely important factor in our own employ-

ment policies even though we might keep wages down and exports a bit higher if we had longer lines of men waiting at the factory gates, so the price and income position of American farmers relative to other producers in this country is a primary consideration, but not the only one, in the farm policy of a nation which consumes 85 per cent of its farm products at home.

In saying this, I do not wish to argue that the contradictions with freer trade policy implicit in the wheat, cotton, and feed grains export subsidy programs are not important. Rather, they are not the only considerations which matter. I cannot immediately agree with the first part of Mrs. Farnsworth's dictum on this score, which was:[4]

In any case, both the price support level and the timing of changes in that level should be based on world supply-demand conditions, crop adjustment timing factors, and the international responsibilities and commitments of the United States—not on farmer welfare considerations.

This was followed in the same paragraph by an important qualification:

. . . The individual farmer income problems that result from lowering the government's present artificial price support for wheat should be dealt with separately, in ways that emphasize constructive employment adjustment of high cost producers and declining, terminable income assistance without any government intervention in wheat pricing, planting, or marketing.

If there were any assurance either that the severe farm income problems which would be generated would be compensated for, or that there will be adequate constructive employment opportunities for farm producers concurrent with the stepping up of the already intense pressures to leave the farm work force, the opposition of farm producers themselves would be blunted. Providing this assurance to potentially displaced farm people through efforts to speed the rate of economic growth and job creation should be an integral if indirect part of the farm policy, whether farm prices are lowered substantially in the next decade or not.

If the price support and price level for wheat were to be based on world supply-demand conditions in the next few years, it would be near $1.00 to $1.20 per bushel on farms in the U.S. Such wheat prices, coupled with comparable prices for other major export commodities, cotton and feed grains, would result in severe income losses to farmers and to substantial changes in asset ownership within a few years.[5]

Certainly there are producers who can sell wheat and cotton profitably at those prices. And there are technologies to be adopted which would lower costs even more on the much larger farms which would result. But the end would not be in sight when the first wave or two of farmers had departed and the early rounds of asset reorganization had

[4] Helen C. Farnsworth, "American Wheat Exports, Policies, and Prospects," *Food Res. Inst. Studies*, May, 1960, p. 275.
[5] Joint Econ. Com., *Economic Policies for Agriculture in the 1960's* (U.S. G. P. O., 1960).

been completed in agriculture. The end product of such a process is a commercial agriculture much different than today's, even though it might still be organized around family farms. It is, I suspect, an agriculture not only of large and highly productive farms but of effective private or group control over market supplies and prices—a curious result when it is recalled that the process which would have brought it about was justified in the name of rejection of public management of market supplies and prices of farm commodities.

We have nothing to fear, perhaps a few decades from now, from an agriculture in which control, if not production, would be concentrated in a few hands. If that occurs, agriculture can be regulated in the public interest as other industries are now regulated. But if an alternative form of control over market supplies of farm commodities and continued need for firm public action is an expected result, there is little to gain from such an adventure.

What are the imperatives for a wheat program in the next five years? What legislative and administrative moves are there which can possibly be made in the political and economic framework we have—not one we would wish for—which will reduce the contradictions, yet meet, in large measure, the objectives of national policy?

The first essential step is to stop the marketing of 100 to 250 million bushels of wheat each year in excess of the amount which can be sold for consumption at any price from about $1.20 to $2.40 per bushel. A second is to reduce stocks of wheat to the point where they are no longer a potential threat to the world wheat trade. Together, these two actions would remove the most serious objections of other wheat exporters to our farm program, and would, in a few years, reduce annual budget expenditures by a half a billion dollars. Before the second—carryover reduction—can be begun, the first—termination of net acquisition—must be accomplished.

There are two basic approaches to the reduction of output and stocks. The first is to reduce wheat prices sharply relative to the prices of other farm commodities; most advocates of this approach would end production control at the same time, either abruptly or progressively. The second approach is to apply effective controls over the production or marketing of wheat at least for a period long enough to dispose of present excessive stocks, while maintaining a level of wheat prices determined by public policy.

The first approach would increase, not reduce, annual additions to government-owned stocks of wheat in the next three to five years if a price supporting loan were continued. Annual wheat production would not only continue at high levels if acreage controls were ended and price supports cut by one-third, to $1.20 per bushel at the farm, but would

probably increase by 10 to 20 per cent from the present. This is indicated by the relative yields of wheat and alternative crops in specialized wheat areas and is supported by surveys which show that at such lower prices, producers would plant more wheat than presently permitted under the acreage allotment program, especially in the Great Plains and the Northwest. This may be a point of fundamental disagreement in economic analysis between the proponents of the lower price and the marketing control approaches. I have not seen evidence suggesting that wheat production would soon decline in response to such lower prices. There is considerable evidence to the contrary.[6]

Production at such low prices could not be carried on indefinitely by the same growers now producing wheat, but it would be done by them for the next four to five years and by their successors after that. More wheat in CCC hands—not less—would be the result if acreage and marketing controls were relaxed at all.

Even at wheat prices comparable with the early thirties in terms of purchasing power, direct control over marketings would be the critical factor in reducing stocks of wheat by 1965. Add to this the "fact of life" that if wheat prices were to be substantially reduced, they would, at best, be stepped down slowly while other farm prices would probably be reduced, also, and it is seen that we would face a situation in which prices at the farm during most of the next five years would not be low enough to cut into wheat or aggregate farm production, but just low enough to make the farmers mad. This was recognized belatedly by the Eisenhower Administration, which indicated in its 1960 message to Congress (on agriculture), that effective control over acres or bushels of wheat would have been acceptable if it had been accompanied by reduced wheat prices.

The fears and objections of competing export nations would be heightened with the growth of wheat stocks under the program described, and the budget item for farm programs would remain very high, although perhaps below today's levels. The modest inconsistency of our export subsidy for wheat in a world of import restrictions and tariffs would be reduced by lower prices, but unless large-scale compensation were paid, domestic farm income objectives would have to be revised downward greatly.

No matter what one's view of the proper level of wheat prices, of the desirability of minimizing government action in the economy in the longer run, or of the ideal economic organization for agricultural production once the surplus stocks are gone, the most appropriate course

[6] Charles W. Nauheim, et al., Wheat Production (U.S.D.A. Agric. Inf. Bul. No. 179, 1958); John A. Schnittker, Wheat Problems and Programs in the U.S. (Kans. Agric. Exp. Sta. Bul., in press, 1960).

to plan for in the next four or five years is to make certain that the greatest contradictions of all, the annual excess of marketings over utilization, and the excessive carryover, are ended. The fact that consumers now pay a bit higher prices for wheat for food than if our wheat were at the world price is a modest paradox compared with the fact that even with the wheat price cut by one-third, taxpayers would continue to buy new wheat for storage and to pay carrying charges on both old wheat and new for an indefinite time in the absence of effective controls over marketings.

If the alternative approach—a program of effective control over either wheat production or marketings or both—were begun, the question of the level of wheat prices to be administered would remain. Logically, it would be settled by the choice of public policy relating to the comparative incomes of commercial farmers and others and, since an export subsidy is involved, by other demands upon the federal budget. If the problems of economic growth, education, and unemployment were being solved, the poor in agriculture, never deeply interested in farm prices and price supports, would lose all interest in them. And with the decline of the false concern for the poor in agriculture as the beneficiaries of farm policy, the argument for a farm price level much higher than today's would largely dissolve, for the net incomes of average commercial farmers could be increased substantially in the early sixties by very modest increases in commodity prices.

Discrimination on the part of commercial agriculture in its price aspirations can hardly be overstressed. I believe that the American public is already convinced—or can be convinced by a strong President—that it is reasonable to provide favorable output, price, and income policies to insure a prosperous and flexible agriculture despite the contradictions involved. But the level of administered prices is important; two self-interests are opposed in that choice. The general public, which lacks direct influence in most pricing, and not farmers will probably decide the elusive but very real "fair farm prices" of the future.

Farmers in their search for market power have often been compared with labor seeking collective bargaining rights to raise and protect wage levels and working conditions, and with industry, growing and merging and exerting increasing influence over aggregate output and prices. Unlike labor and industry, however, agriculture begins its latest quest for bargaining power with its political power in irreversible decline.

The third major necessity for a wheat program, concurrent with bringing annual marketings and the carryover of wheat into line, is to establish the degree of usefulness of farm products in our foreign policy and to change the justification for farm commodity shipments from surplus disposal to economic development. With domestic food and cash export

needs given, the extent to which our wheat stocks and productive capacity have a legitimate role in foreign aid will determine the need for current marketings of U.S. wheat while the carryover is being reduced. This, in turn, is closely related to the domestic income objectives of the wheat program, which depend not only on the price level but also on total marketings in any crop year. A high rate of disappearance of wheat from current production would further reduce the need for higher prices to meet domestic farm income goals.

If the decision in this matter is that wheat and other farm commodities should have a comparatively limited role in foreign aid (apart from surplus disposal), the adjustments due—especially in the production of wheat, cotton, and oilseeds—will be far more difficult than now foreseen.

Wheat is a relatively simple and homogeneous commodity. Because it has limited alternative uses, like tobacco and cotton, it is a commodity which lends itself relatively well to direct limitations on marketings. But a decision to try to establish a bushel marketing quota program for wheat as the most feasible means of reducing wheat stocks and marketings would require facing up to a series of problems relating to the location of wheat production, the character of large producing regions, the composition of the current wheat carryover, and the disposition of resources, chiefly land, no longer needed for wheat production.

About 15 million acres in the Great Plains were first cultivated between 1943 and 1950, under the attraction of high wartime prices, exceptional weather conditions, and the assurance of good prices in the postwar period. Two-thirds or more of this acreage was in hard red winter wheat areas. The seeded acreage of hard red winter alone increased 15 million acres from 1944 to 1949. These changed comparative acreages of hard red winter and other classes became the basis for acreage allotments from 1954 to 1961. Concurrently, higher yielding hard red winter wheat varieties displaced soft wheat in the western Cornbelt, and soybeans became exceptionally profitable, further reducing soft wheat acreage.

The result is seen in the fact that 80 per cent of the wheat carryover on July 1, 1961, will be of the hard winter class. Virtually all the addition to carryover in 1960-61 is of that class, and nearly all additions, so long as the present program is effective, are expected to be of that class.

Discussions of an improved marketing quota program for wheat have centered on uniform reductions in sales of all classes of wheat, of 10, 15, or 20 per cent from recent levels. The amount of wheat to be taken out of government stocks each year would be specified in advance. Together with total expected disappearance of wheat, it would determine the size of the marketing quota. This would mean marketing perhaps 950 million bushels each year, whereas recently about 1,150 million bushels

have been sold off farms each year, 1,000 million for use and 150 million for storage. Testimony before the Senate Committee on Agriculture and Forestry a few years ago indicated that a small reduction of 5 or 10 per cent in acreage or marketings in soft red winter and eastern white wheat areas would not create any problems which could not readily be solved by small shifts in classes of wheat planted, but that a larger reduction might result in a shortage in domestic and cash export markets.

Despite the lack of precedent in wheat programs, unequal reductions in marketings among regions ought to be explored when a new marketing quota program is written. Publicly-sponsored retirement of land from grain, which has been a part of the marketing quota programs for wheat discussed to date, would also be concentrated in certain regions, especially the Great Plains, under such a program, not distributed proportionally in wheat growing regions. Ideally, it should not be simply "land idling" but useful land-use planning. This would raise difficult problems of community adjustment and would surely require heavy compensation. But progress in changing land use in the Great Plains and elsewhere is not impossible.

Unequal regional reductions in wheat marketings would make the already difficult problem of allocation of marketing quotas among regions even more complex. But the comparative overexpansion of wheat production in the Great Plains and the Northwest is a matter which will have to be faced, whatever form the wheat program takes, and it will be no easier if it is postponed for a few more years. Yet, at the start, separate consideration of regions is probably not essential for an improved wheat program.

In summary, we need precision in adjusting wheat marketings to domestic and world needs in the next five years, not hopeful but faulty expectations that farmers would individually reduce wheat production if prices were cut and production controls lifted.

I am willing to leave it at that for the next five years, even though I feel sure that unless farm income objectives are revised downward greatly, we do have a permanent need for government farm programs.

THE PROBLEM MULTIPLYING EFFECTS OF SPECIAL WHEAT PROGRAMS

By Helen C. Farnsworth
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The phrase "special wheat program" is here used to cover any type of government intervention that differentially strengthens or weakens incentives to produce, use, or store wheat, as compared with competing or substitutable commodities. Many historical examples of the problem multiplying effects of special wheat programs could be cited, not only for the United States, but for other countries as well. Here, however, I shall confine attention to special American wheat programs and problems of the past eight years, emphasizing some of the less well-known problems that deserve consideration in planning future legislation.

The American Wheat Surplus. The top-ranking American wheat problem today is surplus production. Carryover stocks have zoomed upward by a billion bushels over the past eight years; and the Department of Agriculture indicates that another 450 million are likely to be added by 1965 if cost-price relationships, export programs, and other major factors remain unchanged [1].

As big as these leftover stocks are, they represent only part of our huge surplus production. While a billion bushels of wheat were being pushed into government storage over the past eight years, another two billion were being shipped out under "special" export programs primarily designed as surplus disposal measures. Moreover, at existing prices, another billion bushels would have been produced if our costly acreage and marketing controls and Soil Bank had not existed. When combined, these figures suggest that in framing new wheat legislation we need to think in terms of an efficient wheat industry which, unfettered, would produce almost half a billion bushels of surplus wheat annually at recent price and price-cost relationships. Such an annual surplus would be almost as large as our total domestic use of wheat and over three times as large as our "commercial" wheat exports.

Regional Concentration of the Surplus. Although it is traditional for economists to regard wheat as a uniform product, this was much more sensible in Adam Smith's time and country than it is today in the United States. Here we must deal realistically with four major classes of wheat grown under different climatic and economic conditions in different geographical regions.

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In the moister areas east of the Mississippi, soft winter wheat predominates. It is grown on small farms, in rotation with corn, oats, soybeans, and other crops. In that region, wheat is definitely a subsidiary crop, primarily complementary but also competitive, and its total acreage has been declining for many years. Even the unusually high relative wheat prices of the past decade have not resulted in any sizable surplus of eastern soft wheat, the bulk of which is used domestically for cake, pastry, and cracker flour.

In the drier Great Plains states hard spring wheat is dominant in the North, and hard winter wheat in the South. These high-protein "hard" types are preferred for breadmaking. They are typically raised on large to very large farms, the size of farms increasing and yields per acre decreasing and becoming increasingly variable from East to West. If the eastern fringes of these two regions are excluded, wheat has few, if any, profitable crop competitors when wheat prices are relatively high; but if wheat prices are relatively low or if wheat controls exist, then grain sorghum and pasture and livestock represent reasonable alternatives in the South, and flax, feedgrains, and livestock and pasture in the North.
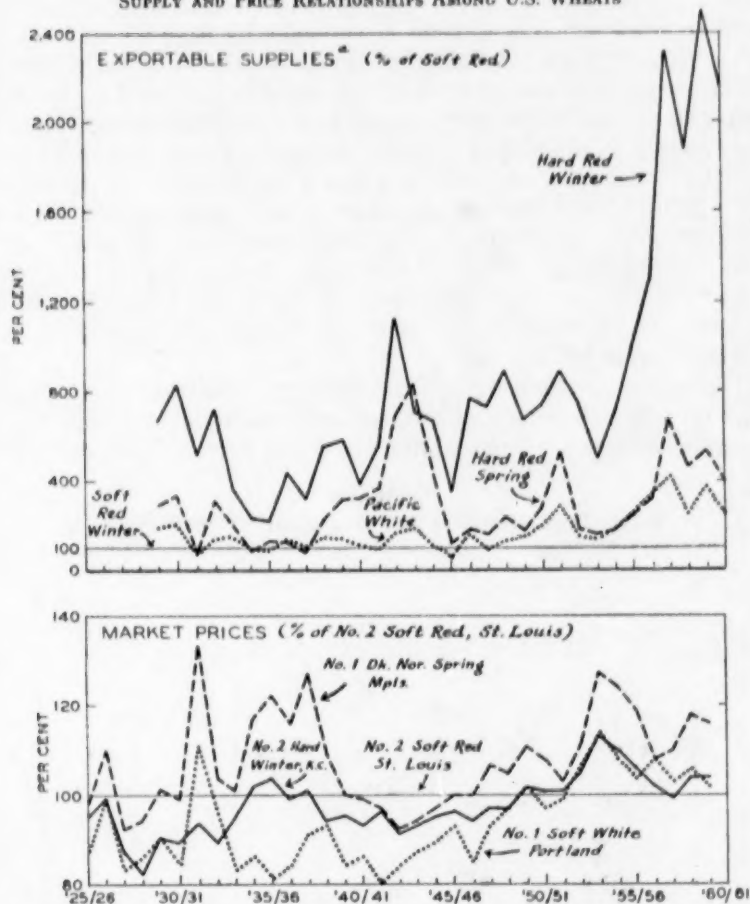
The fourth major wheat region is the Pacific Northwest. That area has the largest, most heavily mechanized, and most prosperous farms, with soft white wheat as the dominant crop. There fallow is widely used in rotation with wheat, though pasture, feedgrains, and peas are also important. In the Great Plains and the Pacific Northwest, impressive economies of scale have been and still can be gained by expanding the size of farms with increased and improved mechanization. The persisting historical trend in these regions has been toward fewer and larger farms with higher net incomes per farm. Pacific white wheat, like hard red winter, but unlike hard red spring wheat, has long been heavily dependent on exports and has historically been one of the nation's surplus problems. Recent carryovers of Pacific white wheat, however, have not been exceptionally large, primarily because postwar expansion of Pacific wheat production was quite modest as compared with that of hard red winter wheat and partly because Public Law 480 has provided a preferred export channel for Pacific white wheat.

Thus, our wheat surplus problem today is concentrated predominantly in the southern Great Plains and secondarily in the Pacific Northwest: no sizable surpluses have existed of the soft wheats of the eastern states or of the hard spring wheat of the northern Great Plains.

Perverse Price Relationships Among U.S. Wheats. Despite the enormous increase in the surplus of hard red winter wheat, its market price has recently stood above that of soft red wheat, whereas in prewar years much smaller surpluses had been reflected in substantial price discounts. Charts I and II show the abnormal shift in wheat price-supply

CHART 1
SUPPLY AND PRICE RELATIONSHIPS AMONG U.S. WHEATS

EXPORTABLE SUPPLIES^a (% of Soft Red)

Hard Red Winter →

Hard Red Spring

Soft Red Winter

Pacific White

MARKET PRICES (% of No. 2 Soft Red, St. Louis)

No. 1 Dk. Nor. Spring Mpls.

No. 2 Hard Winter, K.C.

No. 2 Soft Red St. Louis

No. 1 Soft White Portland

^{a} Crop year exports plus year-end carryover.

relationships that occurred with the strengthening of effective govern-
ment price controls after the Korean war. There is good reason to sup-
pose that these abnormal price relationships have contributed to expan-
sion of the disturbing surplus of hard red winter wheat—a view sup-
ported by the eastward and northward extension of hard red winter into
Missouri, Illinois, and Iowa, where soft red wheat had previously been
grown.[1]

[1] Although this geographic shift partly reflected the development of higher yielding hard
winter wheat varieties, the new varieties would not have been planted so extensively if
government programs had not kept the prices of low-protein hard red winter wheat more
attractive than earlier in relation to soft red.

Three government programs have been jointly responsible for the perverse price relationships of the past decade. First, the terminal loan rates or price supports warrant attention. Why have government administrators kept the price support spreads among the different types of wheat so uniform and so narrow, seemingly in disregard of the huge surpluses of hard red winter and of the very different prewar record of price-supply relationships? I think this mainly reflects one of the universal problems of government pricing in the Western world: there is a general tendency for administrators to keep price spreads between different types and qualities of wheat much narrower and less variable than existing supply conditions and earlier free-market records would seem to warrant. Such tendencies are probably due partly to political pressures favoring equality of producer benefits, and partly to legal and bureaucratic rigidities.

The second government program partially responsible for the changed price relationships among American wheats has been our special export operations. More hard red winter wheat has been shipped out under

CHART II

SUPPLY-PRICE RELATIONS: HARD VERSUS SOFT WINTER WHEATS*
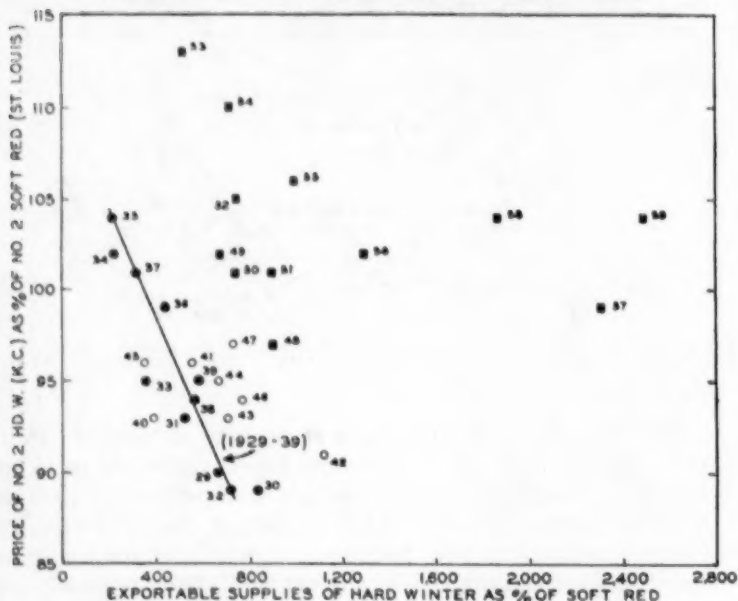
* Numerals indicate the initial year of the crop-years represented.

these surplus-disposal programs than any other type (though in relative terms Pacific white wheat has benefited most).

The third government price influence has been the so-called "International Wheat Agreement subsidy" which since 1954 has actually served as a general export subsidy, applying to all wheat exports. On an annual basis, this subsidy has recently averaged 55 to 80 cents a bushel— roughly 30 to 40 per cent of the corresponding national average price received by American farmers. It has been flexibly controlled and frequently varied; and the specific subsidy rates have been highly differentiated by wheat type. Obviously, these diverse rates have been set with a view to adjusting a group of artificial domestic wheat prices to a semi-cartelized international pricing pattern; and the resulting subsidy relationships suggest more questions than answers.

When the price supports, special export programs, and general export subsidies are all added together, there is no question that their over-all price effect has been perversely to favor the two wheat regions producing the greatest surpluses—and most notably the hard red winter wheat region.

The Fifteen-Acre Exemption Problem and Conflicting Regional Interests. A different type of regional wheat problem is the fifteen-acre exemption issue. Since 1941 farmers growing no more than fifteen acres of wheat have been exempt from marketing quota penalties. This has mainly benefited farmers east of the Mississippi, where roughly 80 per cent of the wheat-growing farms have allotments of fifteen acres or less. In recent years, when wheat prices have been unusually high in relation to corn and oats, more and more small farmers have taken advantage of this exemption. Their wheat sowings and allotments have expanded, adding something like 100 million bushels of wheat annually to the total supply and necessitating a corresponding reduction of acreage allotments for larger farms [2, page 175].

Although almost 60 per cent of this added production has been eastern soft wheat, millers in the eastern area have successfully argued that most of the increase was needed to insure year-round mill operations and traditional commercial exports of eastern soft wheat products [3, pages 211-29]. On the other hand, government officials have been disturbed by the tendency to maintain or increase wheat sowings on the small, high-yielding, high-cost eastern farms—a tendency out of line with historical trends. Even more disturbed have been producers in the Great Plains and Pacific Northwest, who contend that their own wheats could satisfactorily supply a much larger fraction of the wheat milled for pastry and cracker flour, that new milling techniques have greatly

increased substitution possibilities, and that wheat controls should apply to all producers who benefit from the supported prices.

The fifteen-acre exemption now looms as an even more important problem for the future. Proponents of higher price supports with intensified controls dare not leave unchanged the fifteen-acre exemption. Many small farms that have not taken full advantage of that exemption in the past would surely do so if the wheat-support price available to them were materially raised under either a one-price or a two-price system. On the other hand, any proposal to cut this exemption would run directly into the question of voting rights, now denied to small producers in wheat marketing quota referendums. Could Congress be convinced that half a million small producers of eastern soft wheat should be subjected to increased control or new penalties even though they have not materially contributed to the nation's burdensome wheat surplus? And even though local mills might be seriously handicapped? Would Congress agree to added restrictions on small producers without giving such producers a right to vote in marketing quota referendums? The complexity of this whole question and the vital regional conflict of interests involved are well indicated by the following comments of Oklahoma Congressman Albert [2, pages 202-03]:

. . . if you leave it to a referendum, and you leave to those who are commercial growers, under the present definition of the law of more than 15 acres, you will get one answer. If you leave it to all of the wheat growers who grow 5 acres, 10 acres, 3 acres, and so forth, you will get another type of answer . . . you would not have a chance in the world of putting over, say, the National Wheat Growers' proposition if you let all of the growers vote in a referendum.

The conflicts of interest between wheat producers in different regions go far beyond the fifteen acre exemption issue. All producers of hard red spring and soft red winter wheats could reasonably object to controls that would cut their wheat marketings by the same percentage as hard red winter and Pacific white wheats—the true "surplus" wheats. A uniform 25 per cent cut would put average marketings of hard red spring and soft red wheats below the quantities required for domestic use and would allow nothing for customary exports. In contrast, the same percentage cut would leave enough hard red winter wheat and Pacific white, not only to supply normal "commercial" demands for those wheats, but also to cover the associated commercial shortfalls of hard red spring and soft red wheats. Such regionally discriminatory government controls could hardly be tolerated. Moreover, similar regional difficulties would arise in apportioning the preferred "primary" or "marketing" quota under any two-price plan.

I have developed at length the multiple problems related to wheat class differences, not only because they are important, but also because they serve as an understandable example of a host of complex commod-

ity problems that you normally do not hear about—price and supply relationship problems that make administrative pricing and controls on a commodity basis so unbalancing and disturbing.

Abnormal Wheat-Feedgrain Relationships and Domestic Use. We turn now to the distortions in wheat-feedgrain relationships brought about by the present wheat program. During the past five years the amount of wheat used domestically in this country has been smaller than at any time during the preceding quarter of a century—this, despite an increasing population and a revolutionary expansion of the poultry industry (for which wheat has long been a preferred feed). As compared with the wheat-surplus period of the early thirties, the decrease in annual domestic use of wheat exceeds 100 million bushels or 15 per cent, whereas the domestic wheat surplus has increased roughly a billion bushels or 350 per cent—a most abnormal relationship. Since food consumption of wheat has remained practically unchanged from earlier levels (despite lower per capita consumption), the recent decline in domestic use is wholly attributable to reduced seeding and feeding. These changes, in turn, have been jointly due to our various special wheat programs, which diverted former wheat acreage to uncontrolled and semicontrolled feedgrains (reducing seed use of wheat and raising the number of feed-units produced on the diverted land) and which supported wheat prices at abnormally high levels in relation to feedgrain prices (thus reducing feed use of wheat).

The abnormal price-acreage relationships that have recently existed between wheat and feedgrains are shown in Chart III. During the last five years, corn and oats have had a lower domestic purchasing power relative to wheat than in any similar period of the past half century. Yet this big price discount on feedgrains has been associated with the maintenance of an unusually high rather than low ratio of feedgrain to wheat plantings—a reflection of the government's wheat acreage and marketing controls. These facts and the associated pile-up of surplus feedgrain stocks since 1954 clearly suggest that all legislative proposals for a new wheat program should be closely scrutinized from the standpoint of the entire livestock-grain economy.

American Competition in "Commercial" Wheat Trade. So far attention has centered on domestic problems, which are only part of the picture. The American wheat program has multiplied economic problems for other countries as well as our own, has influenced world export pricing, and has seriously weakened American leadership in sponsoring freer, fairer multilateral world trade.

Until 1950 or even 1952 the enlarged postwar exports of this country, generously financed by UNRRA and Marshall Plan grants, were badly needed to meet food deficits abroad. But subsequent agricultural re-

CHART III

PRICE AND ACREAGE RELATIONSHIPS: WHEAT AND OTHER GRAINS
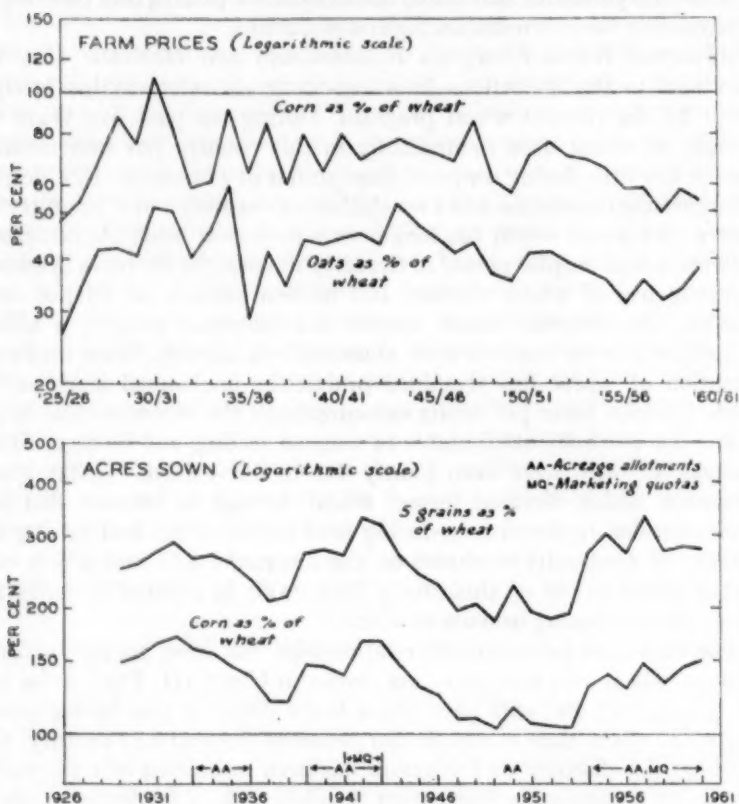
covery and expansion in Western Europe and a couple of bumper wheat crops created an international wheat surplus that has persisted and worsened. With the surplus came a change in American wheat export policies and an associated shift in international attitudes concerning them. Unstinted praise yielded to deep concern, mingled with varying and increasing degrees of disapproval.

To understand recent American export policies and problems, it is desirable to look first at the so-called "commercial" exports, purchased with dollars by commercial importers like the United Kingdom and Switzerland. In prewar years, all exports were commercial, and the prices were determined in a world network of sensitive, freely adjusting futures markets. Since World War II, however, commercial export prices

have been government administered and partly subsidized, with the dominant price decisions reflected in the price quotations of the Canadian Wheat Board and in the export subsidy rates set by American administrators.

Through July, 1953, the American export subsidy system was operated as part of an internationally approved wheat pricing program tied to the 1949 International Wheat Agreement (IWA). Importing countries obviously benefited; and other exporting countries did not complain because their own exports and export prices were generally satisfactory, and no disturbing surplus existed until the very end of the period [4, pages 217-48].

International problems quickly mounted, however, and complacency gave way to increasing intergovernmental friction after 1953, when a second bumper wheat crop was harvested in North America and a truncated new wheat agreement became effective—an agreement which the United Kingdom refused to sign because of the "excessively increased" maximum price. Under these changed conditions, the Canadian Board gradually lowered its wheat-export price; and the United States IWA subsidy was extended to nonagreement exports as well. Then the problem was to keep this general export subsidy so adjusted that it would continually move out a satisfactory amount of commercial exports in competition with other exporting countries. The position of American administrators was and has remained exceedingly awkward. On the one hand, they have faced domestic pressures from mounting wheat stocks and from Congressional demands for expanded exports. On the other hand, they have been in no position to push competitive pricing strongly in the export field since that would mean fighting the unsubsidized (or modestly subsidized) wheat producers of Canada, Australia, and Argentina with export subsidies drawn from the unmatchable American Treasury.

The deep-lying resentment of other exporting countries against American subsidies is one of the most important "facts of life" in the world economy today. Official representatives of competing exporting countries have continually contended that the world's wheat surplus difficulties are primarily attributable to the unsound American wheat program, that the United States government should therefore hold or get rid of its surplus wheat in a way that would not shift to competing countries any part of the cost of its errors, and that any extension of American commercial exports beyond their traditional level represented unfair trading and an infringement of American obligations under GATT.

Under these circumstances, American administrators turned to in-

creased negotiation and co-operation with Canada. But this did not help much. Most historical price and export patterns for wheat favor the Canadians. And no trustworthy, modern economic guide exists for determining appropriate Canadian-American price relationships, or the "fair" or even "normal" share of each country in the world's present commercial export trade. Moreover, the resulting close co-operation between the world's two major wheat exporting nations has been looked upon with suspicion, bringing occasional charges of "cartel pricing." It has presumably raised prices to commercial importers and strengthened the desires of importing countries to minimize their dependence on foreign wheat.

American "Special" Wheat Exports. Even greater than the problems relating to American commercial wheat trade are those pertaining to our special or concessional exports. The special exports have consisted of three types: food-relief grants and donations, barter transactions, and sales against foreign currency payments. Let me mention a few of the problems associated with each.

American food-relief grants and donations are usually distributed directly and free of charge to consumers who face serious food-shortage emergencies or who otherwise are in special need. Partly because these free supplies have been small (only 25-35 million bushels annually) and have been carefully handled by the International Cooperation Administration, they have won widespread international approval. Obviously, they should be continued. Several problems have emerged; but the primary one is the persisting question asked by many sincere Americans: "Since we have so much unwanted surplus wheat, why don't we give larger amounts to feed the hungry millions of the poorer countries?"

The chief answer is that except when faced with critical food-shortage emergencies (usually unforeseen), the governments of most underdeveloped countries do not have the facilities for, nor do they want to encourage, extensive free-food distribution—particularly distribution that depends on the continuation of uncertain foreign food grants or, alternatively, on their own purchases of food. The recipient governments correctly envisage associated disturbing problems: e.g., the tendency for much of the food relief to be distributed by religious societies that often promote their own religious views at the same time; the probability that pilfering and mismanagement of relief supplies would assume disturbing proportions in broader community programs; and the certainty that a sizable percentage of the recipients of free wheat or flour would rush to local merchants to exchange their prized gift for an assorted basketful of more wanted products, thus pushing the gift wheat into commercial channels and lowering grain prices to local pro-

ducers. Such considerations have kept free-food distribution programs small in the past and are likely to do so in the future.

The second type of American concessional wheat exports—a peculiar brand of barter trade [5, pages 238-43]—has been vigorously condemned by competing exporting countries as one of the most objectional forms of unfair competition in commercial import markets. When American barter sales rose to a peak of 87 million mushels in 1956-57, Canadian protests were so loud and evidence of displacement of commercial trade was so convincing, that major reforms were made in the basic program. These have sharply contracted the total volume of barter wheat trade, channeling most of it away from major commercial markets. Such exports now account for only about 20 million bushels of exports annually; but even this figure is excessive, judged by approved trade standards.

The barter program has created problems not only for competing grain exporting countries but also for some of the raw material exporting countries that temporarily benefited from American barter stockpiling. Obviously, the artificial stock-pile demand could not be indefinitely sustained. Moreover, the great bulk of the bartered materials have been channeled not into our semipermanent "strategic stock pile" but into the debatable "supplemental stock pile" (especially created for the purpose) which holds a greater threat of liquidation.

The third type of American concessional exports—sales against foreign currencies—has recently dominated the nation's wheat export picture. Arranged mainly under Public Law 480 (the Agricultural Trade Development and Assistance Act of 1954) and to a minor degree under Section 402 of successive Mutual Security Acts, these concessional exports last year rose to a maximum 85 per cent of the nation's special wheat export trade. And the special exports, in turn, were more than three times as large as the subsidized commercial exports.

Over the past six years, the P.L. 480 program alone has accounted for over 2 billion dollars worth of American-financed wheat that cost the CCC over 3 billion dollars in price support operations [6]. Expansion of this concessional trade has represented a major national budgetary outlay. It has also brought many economic and international political problems difficult to assess. In view of the widespread misconceptions that prevail concerning these exports, let me indicate what they do not do, as well as some of the major problems they pose [5, pages 230-44].

1. Despite humanitarian claims to the contrary, such wheat is not used to "feed the hungry in the poorer countries." It is true that the great bulk of P.L. 480 wheat goes to the less-developed countries. How-

ever, it is not distributed free nor sold at discount to the "hungry" poor,
but rather is typically sold through ordinary market channels to the
well to do, who can afford this luxury cereal.

2. P.L. 480 wheat does not add correspondingly to world grain ex-
ports and consumption: part of it displaces commercial exports. Despite
efforts of the Department of Agriculture to prevent P.L. 480 wheat
from "displacing U.S. usual marketings" or from "unduly disrupting
world prices or normal patterns of trade with friendly countries,"
practically all grain experts would agree that a substantial loss of com-
mercial export sales has resulted from this program. Even if the loss
does not exceed 15 or 20 per cent of the P.L. 480 exports, it can be very
serious, indeed, for the few friendly exporting countries whose com-
mercial trade is most affected; e.g. Australia and Burma (Asiatic
trade) and Argentina (Latin-American trade). Moreover, Canadian
commercial grain exports have been cut indirectly and less obviously
in Western European markets, where increased competition has come
from the Soviet Union as a result of grain freed from Eastern European
import markets by P.L. 480 exports to Poland and Yugoslavia. Finally,
in some year, P.L. 480 wheat deliveries to certain countries appear to
have freed exports from the same countries of rice, barley, or other
grain; and, contrariwise, sales of P.L. 480 rice and corn to certain
countries have freed wheat exports from those countries. Obviously, the
commercially competitive effects of the P.L. 480 program are much
more complex, widespread, and diverse than is commonly recognized.
These are the effects that are so strongly feared and resented by com-
peting exporting countries; these are the effects that are likely to in-
crease sharply if American policy moves in the direction of expanding
P.L. 480 export disposals.

3. Such surplus wheat does not contribute as much to economic de-
velopment and inflation control in underdeveloped countries as is com-
monly claimed. Some sponsors insist that "P.L. 480 wheat is just as
good as foreign aid dollars." Obviously this is not so, since dollars can
be used not only to buy food but also needed capital equipment. In-
deed, P.L. 480 wheat operates as an effective substitute for dollars only
to the extent that it runs counter to the basic concept of the program
and displaces commercial food imports. If displacement of commercial
imports is disregarded, the chief contribution that P.L. 480 wheat
makes to economic development is to add modestly to the total supply
of economic goods in the recipient countries and to channel to the par-
ticipating governments (as American loans) much of the domestic
money spent for P.L. 480 wheat by local purchasers. At best, the num-
ber of underdeveloped countries appreciably aided in this way has

been small, since each year roughly 90 per cent of P.L. 480 wheat has gone to the six largest recipients [5, pages 231-33]. Moreover, there are longer run disadvantages to consider. Insofar as P.L. 480 wheat reduces local grain prices in the recipient countries, forcing domestic producers to shift to less advantageous crops and lessening government interest in raising agricultural productivity and production, economic development may actually suffer in those countries. In any case, the scheduled P.L. 480 loan and interest repayments are bound to become an onerous burden for some recipient countries, carrying the threat of retardation of development progress, unless large portions are returned in the form of grants.

4. For the United States, the P.L. 480 program poses serious additional problems. Increasingly that program has cast disturbing shadows of unprincipled short-run expediency, deceptive exaggeration of food and development benefits, concealment of financial losses, curtailment of commercial import markets, intergovernmental friction, and excessive foreign currency holdings that can never be collected [7].

An interesting side question is what price the United States has received for its P.L. 480 wheat. If one makes the fanciful assumption that all outstanding foreign currency loans will be fully repaid on time in convertible currency of unchanged value, one might put the present discounted price of recent P.L. 480 sales at about $1.10 per bushel, basis Kansas City, No. 2 hard winter. But if, more realistically, one assumes only 60 per cent loan repayments, the indicated price would be 65 cents a bushel—roughly 40 per cent of the commercial export price and a mere fourth of the estimated price support cost of the wheat to the CCC.[2]

Financial Cost of the Present Wheat Program. Chart IV summarizes some of the principal cost items. It includes no allowance, however, for several very sizable but not easily quantifiable costs. Thus omitted are allowances for subsequent nonpayments and reduced payments of P.L. 480 loans, for depreciation of foreign currencies yet to be received, and for the large costs that will result from continued storage and eventual liquidation of the present enormous CCC stocks. These three excluded "costs" will add up to large sums over the next few decades.

The central feature brought out by Chart IV is that when we add together the incomplete, most readily calculable costs of the present wheat program—the extra costs imposed on consumers, the realized costs borne by the public treasury, and the initial concessional discounts

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[2] Calculations by T. W. Schultz, based on different assumptions and applicable to all P.L. 480 commodity exports, indicated a net over-all return to the U.S. of only 10-15 per cent of the corresponding CCC costs.

CHART IV

SOME COSTS OF WHEAT PROGRAM VERSUS FARM CASH RECEIPTS
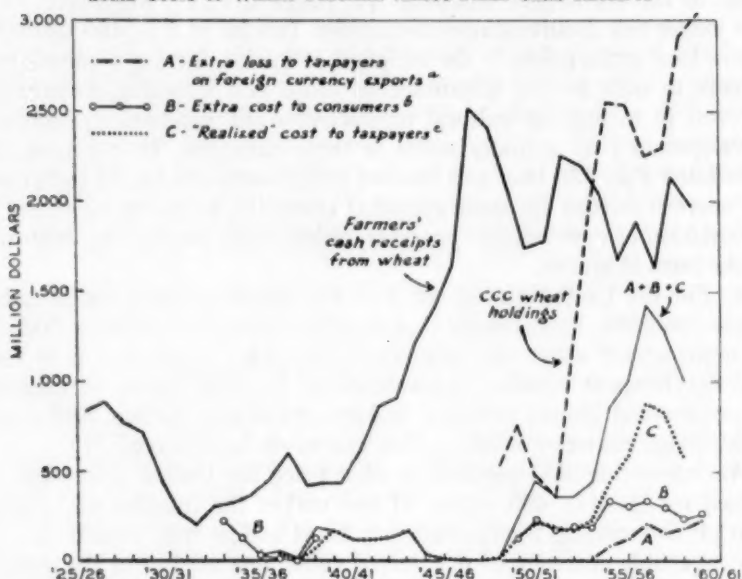
* Difference between estimated concessional price on foreign currency exports and commercial price (assuming loan repayments).
ᵇ Difference between domestic price and commercial export price.
ᶜ Includes direct costs of CCC wheat operations (U.S.D.A. estimate) plus estimated share of wheat in general CCC costs.

on wheat sold for foreign currencies—we arrive at incomplete cost figures (line $A + B + C$) that stand startlingly close to the total cash receipts which American farmers receive from their wheat marketings.

A Few Legislative Suggestions. I have mentioned many, but by no means all, of the disturbing economic problems that have developed under our current wheat program. All have had a common problem-multiplier core: administered pricing and controls rigidly tied to an obsolete historical base. No one has known how to modernize that base. Current legislative proposals for still higher support prices and intensified controls move still farther in the same history-bound direction. There has been no trustworthy economic guide to basic wheat values, trends, and relationships for more than two decades—a period characterized by revolutionary changes in agricultural technology, in politico-economic orbits, and in consumer demands. International free markets for wheat, or anything approaching thereto, have not existed since the thirties; and that decade was characterized by an unprecedented world depression and a distorting North American drought.

Today no one can say with assurance what wheat price, production,

and export decisions are in line with basic economic trends and what
ones are not. My first, major suggestion for new wheat legislation is
that we adopt the only reliable method known for finding out: that
Congress free both American farmers and American grain markets.
Obviously, this cannot safely be done overnight. It is essential to move
to free markets cautiously and to employ appropriate adjustment de-
vices for easing the pains of transition. Even at the end of the transi-
tion period, it would probably be desirable to maintain a stand-by
minimum price guarantee to producers that would be set low enough
so that it would rarely become operative, and that would gradually
move lower or higher in line with underlying trend values (determined
with reference to moving average export prices, on the one hand, and
to the changing size of year-end stocks and concessional sales on the
other).

For the transition period, three problems require special attention:
the existing surplus stocks; the commercial wheat pricing system; and
farmers' incomes. Let me indicate how I think each of these problems
might be partially attacked.

So far as old-crop stocks are concerned, the crucial thing is to keep
them from competing with current commercial marketings, and, within
that limit, to use them as constructively as possible. It would seem
generally desirable for Congress to earmark 500 million bushels of exist-
ing government stocks as a semipermanent defense and emergency re-
serve. That would leave an indicated surplus of about a billion bushels
for safeguarded surplus-disposal exports and for possible denaturing for
feed.[3]

Priority uses of the surplus would include disaster-relief grants to
foreign governments, donations to American relief organizations for
direct distribution abroad, and such "development" grants and conces-
sional sales as could win the final approval of some appropriate inter-
governmental group—a group that should be heavily weighted with
representatives of competing wheat-exporting nations and, in lesser
degree, with representatives of interested rice-exporting countries and
commercial wheat-importing nations. Moreover, we might hope that
this group would be instructed to co-operate closely with the Interna-
tional Monetary Fund and the International Development Association.
Under properly safeguarded conditions, such noncompetitive surplus-
disposal exports might well average 150 million bushels annually. If so,
the initial surplus stocks could be liquidated in about six years. This
would appear quite satisfactory. Even a longer liquidation period would
be tolerable if new surpluses were not piling up at the same time.

[3] Denaturation for domestic and export feed has long been a favorite method of surplus
disposal in Western Europe. A broader proposal for a new livestock feeding and canned
meat export program has recently been suggested by Karl Brandt [9].

More difficult are the price and related income problems. It seems reasonable to suggest that the national wheat support price (now $1.78 a bushel) be reduced to 90 per cent of the equivalent average export price of the three preceding years (currently about $1.15),[4] that acreage allotments and marketing quotas be removed, and that declining "compensatory" income payments be made to producers over a limited transition period (say, five years). It is important to insist that the compensatory payments be based not on current wheat marketings but on non-penalty marketings of some fixed past period, thus preventing these payments from operating as a wheat-production incentive. In order to ease the associated Treasury burden, such transition payments might be partially financed by a declining processing tax on domestic millings. In no case, however, should this regressive tax be set at a level that would raise the price of wheat for domestic flour above that now in effect; and prescheduled annual reductions should cut the tax to zero by the end of the transition period.

The proposed downward adjustment of domestic wheat prices to a semifree export basis might be made either all at once or gradually over two or three years. If the latter, then acreage allotments and marketing quotas should be maintained until the below-export support level is reached; and compensatory income payments should be withheld from producers who market more than their authorized quotas. Such payments would not be withheld, of course, from producers who market less than their current quotas, or even from those who stop farming completely.

Precisely how and at what level the income payments should be set is a matter on which opinions will differ widely. The decision is bound to be a political one. But what is most important is that the payments be designed and handled so as to promote and not discourage needed economic adjustments, such as reduction of high-cost wheat production, further expansion of farm size and mechanization, and increased migration out of agriculture. Tying the compensatory income payments to a fixed past marketing base would help. Prescheduled reduction of these payments during the transition period would presumably also contribute to more orderly adjustment. But the greatest contribution to such adjustment goals would have to come indirectly from broader national policies and programs—from the maintenance of generally favorable economic conditions, from improvements in education and educational opportunities, from the development of a really good nation-

[4] The support price equivalent of the subsidized export price of any recent year may be taken as equal to the announced national support price minus the national average export subsidy. So calculated, the actual support price equivalents for the past three years (average) and for 1959-60 were $1.25 and $1.19, respectively.

wide network of occupational and employment advising centers, and from similar government activities.

These few legislative suggestions represent no more than the bare bones of a revised wheat program. The indicated changes would eventually give all producers and all governments a much better idea of the true international value of wheat and would eliminate current distortions in domestic wheat-feedgrain price relationships and consumption. In addition, these changes would permit American producers to use their resources more economically and to compete freely and fairly for a larger share of the world's expanding commercial grain trade. Moreover, they would reduce and eventually eliminate the existing regressive "bread tax" on American consumers (which some current legislative proposals would sharply increase). Equally or more important, such legislative changes would remove any justification for foreign protests against our "unfair subsidization" of wheat production and exports, and would greatly strengthen the international position of American officials in pressing for freer multilateral trade and for world-wide reduction of import barriers (and specifically for reduction of excessive agricultural protection in the European Common Market).

Practically all of these advantages would stem from reliance on a modestly limited free-market mechanism. The historical record of the past century clearly indicates that relatively free wheat markets in major exporting countries operate well in balancing world supply and demand with moderate year-to-year price changes (except in times of war, of great economic recession, and of transition from price control). In contrast, administered pricing and marketing controls have established an unenviable peacetime record of exaggerating and prolonging both shortages and surpluses of wheat, of keeping wheat prices out of line with reality for many years, of distorting wheat-feedgrain price relationships and perversely discouraging wheat feeding in a period of mounting wheat surpluses, and of conferring special government favors on domestic wheat producers in certain geographic regions. It is time to return the complex task of pricing wheat, competing crops, and livestock to the free market (modestly restricted as to allowable annual price declines). And it is time for Congress to design farm subsidies and depressed area assistance that will constructively provide the nation's rural (also urban) population with better education, new skills, and employment guidance which will enable them to earn unsubsidized higher incomes by expanding the volume of desired goods and services.

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DISCUSSION

J. Howard Craven: I would be remiss in my duty as discussant if, out of kindness to the first speaker, I failed to comment on what seems to me to be the most striking aspect of his presentation: the rhetoric was much more that of a political speech than that of a professional paper. I am struck by the colored language which pervaded his document, by the overtones of "good" and "evil" on almost every page, by the semantic manipulations conjuring up visions of "longer lines of men waiting at the factory gates" and raising a question as to the future of "family farms" in a free-market wheat economy. In arguing for government controls, he found favor in the pattern of administered prices which he equated, by direct implication if not overt statement, with "wise policy," "justice," "order," "consistency," "democracy," "reason," "inward strength," escape from "tyranny" and from "submission." He similarly equated reliance on government with "progress," "perfectibility," "faith," "Utopia." Views with which he disagreed became "nonsense," "bad management," "no longer tolerable." The "restraints upon individual freedom" which he (possibly) foresaw were "democratic" rather than "totalitarian restraints." The problems raised by the type of program he proposed were virtually written off as "modest," including the higher prices consumers pay for wheat, the limits upon farmers' actions to date, and the inconsistency of our export subsidy program.

Strongly connotative language, however, was not the only aspect which had a political ring. Appeals to other economic groups for support—labor unions and industry—might be justified as the drawing of parallels essential to his case. This is not true, however, of his first major contention that there is an emerging spirit of agreement that farm output and marketings are to be managed and that discussion is narrowing to a question merely of the means of such management. Such a contention deserves outright rejection. Recent literature in the field contains expressions of strong doubts that the direction of our agricultural policy should move toward more governmental management. (See Murray Benedict's talk on "The Farm Surplus Dilemma" before the agricultural section of the California State Chamber of Commerce on December 1, 1960, or Karl Brandt's paper before the Western Farm Economic Association meeting at Stanford University on August 25, 1960, entitled "Guidelines for a Constructive Revision of Agricultural Policy in the Coming Decade.") The election returns in the wheat states do not support the view that farmers want more government in agriculture. The very fact that the second major paper in this session makes a case for moving away from government controls over the wheat economy leaves one wondering how Professor Schnittker could believe that the position he endorses is assured in its general scope and that debate is relevant only with respect to the details of an expanded controlling role of government.

Professor Schnittker is unwarranted in his assumption that agricultural pol-

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icy discussion should realistically be limited to the area near present policy, with suggested changes restricted within the bounds of current political feasibility as he sees it. On the contrary, the function of professional economists and agricultural economists in the realm of public policy, I believe most of them would agree, is to clarify the implications of alternative policies, thus shedding light on the direction in which public policy ought to move rather than trying to predict the direction in which it will move. The function of the legislator or the campaigner is not the function of the economic analyst. Economic insight is seldom popular when it is first voiced, but, if its validity holds, it gains support over the course of years. The public policy analyst's function is to suggest socially responsible guidelines in the hopes of leading, over an indefinite period of time, the thinking of people whose job it is to grapple with the temporizing pressures of current practical politics.

The second major point in Professor Schnittker's paper was that managed output and controlled pricing are characteristic of a large sector of our nonagricultural economy and that, therefore, consistency requires agriculture to develop institutions to manage output and control prices in the public interest. It is equally consistent to seek to stem the movement toward controlled markets in the nonagricultural sector, as well as in the agricultural sector. It is the purpose of antitrust policy to produce freer markets and to reduce the areas of managed aggregate outputs and controlled prices. The argument of consistency among the various sectors of the economy is quite irrelevant, therefore; the basic question is whether one's bias is toward freer markets or toward more controlled markets, and the extension of either bias would give a consistent pattern to the economy.

Nor is the case made by Professor Schnittker for beneficent government very convincing unless one already believes it; again it is a matter of one's individual political prejudices. However, the argument may also be irrelevant on another score. If one were to grant that the government should act to prevent farm income from declining, the major question still remains whether that governmental role should be exerted through the market mechanism or directly to income-bereft farmers through a welfare program. It does not follow that a beneficent government should manifest itself through a manipulation of wheat output, marketings, or prices.

Professor Schnittker's "imperatives for a wheat program" are overparticularized, and the choice he poses between different specified methods of meeting these imperatives is a false choice. The real alternative is missing. If farm policy-makers were to separate in their thinking the problem of rural poverty from the problem of efficient commercial agriculture and were to treat welfare problems of rural people as welfare rather than market problems, the way would be open to solution of some of the more trying difficulties in the farm economy. The alternative to tighter production and marketing controls on all producers would not be the lower pegged price for wheat implicit in Professor Schnittker's paper, but a free market for wheat with no government purchases whatever. If wheat farmers were then given their individual choice as to operating in that free economy, those that preferred subsidized income could be subjected in return to very rigid controls designed not to rig the market but to

remove part or all of their resources out of agriculture entirely and out of their formal control. It is important in moving toward a free economy in wheat to make the policy clear and to make all wheat land to which title or control is transferred in the future subject to free-market operation; thus cutting off clean the growing tendency to capitalize government payments into land values and to multiply the economic problems associated with wheat. With the government ceasing its price support program, thereby ceasing its additions to stocks, the problem of reducing existing stocks can be handled separately on its own merits, and farmer plantings and marketings can become once more responsive to a world market price. Income supplements can be designed so as not to interfere with the market nor to transfer the problem of U.S. wheat farmers to other countries or producers of other commodities.

A vigorous effort to move farm policy in this direction could do much toward solving "the wheat problem" within the next few years.

L. E. FOURAKER: I am what my colleagues in agricultural economics call a general economist; I will keep my comments in the same category.

I would like to examine the hypothesis that our past agricultural policy, including that affecting wheat and Mr. Benson's public image, has been quite excellent if judged by requirements and standards somewhat broader than those usually employed by economists.

Economists have gone to the resource allocation center of the agricultural problem with a directness which should be gratifying to any price theorist, even one who views the world through the University of Chicago. However, there is a risk involved in considering only the core of a problem. Like the weather observer who samples only from the eye of the hurricane, we may be missing a lot of peripheral movement.

As our speakers have indicated, technological change in agriculture, in conjunction with an inelastic demand, has created a maladjustment which implied that a large number of people would have to leave agriculture. The ideal agricultural policy should not prevent such migration, in my judgment. This is a negative requirement, but it is unreasonable to expect Congress to design a system which improves upon the pricing mechanism as an allocater of resources.

The second requirement I would place upon an agricultural policy would be that it neutralize the political potential of the farmers to induce legislation which would keep resources in agriculture. This seemingly is another negative requirement, but it is no mean task. To keep the most numerous producers group in a democracy on the political defensive while their incomes fall sufficiently to force them out of their industry is a stringent demand to place upon any legislative body. If we consider further that the "cultural distance" the farmer has to traverse in leaving agriculture for urban employment is close to the maximum between occupations in our society, the task seems formidable.

These requirements are not trivial. Let me augment one Smithian analysis with another, and look at the record. A graduate student of mine, Mr. Dhillon, has informed me that since 1936 a net of over 20 million persons have left agriculture. Clearly this is one of mankind's magnificent undertakings: it overshadows other well-known mass migrations such as the Great Boer Trek and

the movement led by Moses. Perhaps comparable transitions have occurred in Russia and China, but some of that movement has been feet first. That our migration took place without bloodshed, leadership, or even much general attention is testimony to the power and efficiency of the pricing system. Of course, the American agricultural system was in disequilibrium over this period and is at the present time; otherwise the resources would not have left, and continue to leave, the industry. At each point of time there has been a misallocation of resources, but the over-all performance has been encouraging.

Thus we have evidence that the federal programs in agriculture have not prevented the movement of resources out of that industry. The major determinant of the rate of movement of human resources out of agriculture seems to be the availability of employment in the rest of the American economy, according to George Brandow. The great waves of migration coincide with the expansion phases of the business cycle; recession or lateral movement of the economy effectively reduces the movement out of agriculture. Our past agricultural policy has not prevented the farmer from accepting his urban alternative cost, provided that cost has not been eroded by a slow or negative rate of growth.

I feel the second requirement also has been satisfied. The agricultural programs over the past quarter century have cost more than 20 billion dollars, a fact which has received more publicity than the migration of 20 million people. This cost and the salient absurdities associated with price supports have left the farmer in an apologetic and defensive position with respect to other American citizens. The public impression is that the farmer has been getting too much financial aid rather than too little. Politicians must rely on promises to farmers of more effective programs, not more expensive ones.

For example, in the last eight years, even Republican publications have referred to Secretary Benson's program as a "mess," a "fiasco," and a "failure." The actual Benson program was a continuation of that policy which permitted the pricing system to force people out of agriculture by allowing their incomes to fall. The embarrassingly large costs associated with this program resulted from the slow rate of growth of the American economy and the lack of urban opportunities for farmers. Secretary Humphrey was the villain behind the surplus stock pile, not Secretary Benson.

This view of agricultural policy may be considered as a nonzero sum game. The strategies available to the farm community are: (1) leave agriculture; (2) stay in agriculture. The policy strategies available to the government are: (*a*) do nothing; (*b*) continue the past policy; (*c*) adopt a policy which would raise farm income enough to keep people in agriculture. I would impute a long-run pay-off matrix as shown in Figure 1. The farmer's income if he leaves agriculture is his alternative wage A, shown in column 1; the pay-off to farm strategy (2) is low income L if the government does nothing, medium income M if the government continues the past policy, and high income H if the government adopts an effective policy.

The government's order of preferences would be $A > M > H, L$. This ordering is dictated by considerations of resource allocation and costs, both monetary and social.

If the government can create a growth environment for the economy, then

Government Strategies	Farm Strategies	
	(1)	(2)
(a)	A	L
(b)	A	M
(c)	A	H

FIGURE 1

$A > M$ and the game has an equilibrium at $(b, 1)$, yielding the social optimum. If the indicated rate of growth cannot be maintained, then $A < M$ and $(b, 2)$ is an equilibrium. Thus strategy b is the best for the government under either growth condition (i.e., it dominates the alternative strategies); however, since the $(b, 1)$ solution is preferred to the $(b, 2)$ solution (it costs less and represents a better allocation of resources), it is incumbent upon the administration to induce that growth rate where $A > M$. The best agricultural policy is an expansionist monetary and fiscal policy.

C. ADDISON HICKMAN: These two revealing and somewhat divergent papers on U.S. wheat policy have done much to clarify a tangled situation and to document grave weaknesses in our current program. A clue to one of the causes of the apparent breakdown of our wheat policy is found in Professor Farnsworth's paper, which sketches in some detail the international ramifications and effects of American wheat policy. It is clear that, once again, the United States has perhaps inadvertently and somewhat absent-mindedly made world policy while acting on the assumption that it has made domestic policy alone. As wheat policy demonstrates, the United States is simply too powerful a force in world markets, in the world economy generally, and in the world political structure, to leave out of account in its policy planning the effects of its policy upon the rest of the world. Indeed, merely minimizing damaging effects of a policy upon other nations, although commendable and more than we have sometimes done, is not enough. Wheat is a world problem and requires a world solution; any nominally domestic policy, such as ours, must be an integral part of that world policy. When problems transcend national boundaries, the old dichotomy between domestic and foreign policy, especially in a nation such as the United States, ceases to have much meaning.

Professor Farnsworth's paper demonstrates, as does some of her other work on this subject, that a persistent policy of pricing U.S. wheat far above world prices and above the capacity of foreign or domestic markets to absorb has had seriously disruptive international economic effects. Although domestic wheat stocks have risen steadily, stocks have been held down to their present levels only because we have been able to dispose of a major portion of the surplus abroad. This has been accomplished in part by a general export subsidy, amounting to 30 to 40 per cent of national average prices received by American wheat farmers, and especially by a variety of "special" or "noncommercial" surplus disposal programs. Although such exports have kept some sort of lid on government stocks and have doubtless on occasion alleviated some distress abroad, these exports have also displaced some commercial movement of wheat,

have led to bitter charges of dumping by other major wheat exporters (most of whom are our political allies), have distorted world price and production patterns, and have been of questionable assistance to economic development.

Such considerations as these cannot, perhaps, be the only factors considered in the formulation of U.S. wheat policy. There are doubtless many aspects of policy which must be shaped in terms not only of assisting U.S. wheat farmers, to the degree that this may be consistent with the general welfare, but also of the general impact upon U.S. agriculture and the U.S. economy. Even so, at this particular juncture in history, world-wide as well as U.S. aspects of the wheat problem and world-wide as well as U.S. effects of our wheat policy should be carefully weighed as policy is made. Making policy nationalistically and then attempting to blunt the damage is not enough.

I do not pretend competence to judge between the somewhat contrasting policy proposals set forth in these two papers. Professor Farnsworth urges return, probably after a brief period of transition, to an essentially free-world wheat market. During this change-over period, policy would feature an orderly liquidation of existing wheat stocks, price supports that would merely serve as a disaster floor, possible brief retention of output controls, and steadily declining compensatory payments based on a fixed past period of wheat marketings. Professor Schnittker's apparent preference is for principal reliance upon reduced farm output, through either land retirement or marketing quotas or both, which might well be coupled with quite high prices. In an earlier paper, he has taken a rather dim view of compensatory or direct income payments, especially if not coupled with output controls, and has noted the opposition of farm groups to such payments, in part on political and sociological grounds.

The ultimate decision between such conflicting approaches must be made on broad economic and political grounds, and it is not the function of these comments to prejudge what that decision should or will be. If, however, we are to look upon our wheat policy as part of a world wheat policy or as part of our own foreign economic policy and not solely as a part of domestic farm policy, some of the international implications of Professor Farnsworth's proposal should be given the most careful attention. Either policy proposal, if it worked as its protagonist outlines, would presumably reduce the present overwhelming pressure of government stocks and might thereby somewhat reduce the present volume of U.S. surplus disposal abroad. Professor Schnittker's approach would, however, continue to involve export subsidies, perhaps at even higher levels than at present, for such wheat as would be exported commercially, and some special or noncommercial exports would presumably still be relied upon. Basically, U.S. wheat prices would remain far above world prices, and to the extent that any stocks did accumulate, there would be a powerful tendency to dump these stocks abroad. Under Professor Farnsworth's proposal that we return gradually to the free market, U.S. wheat prices would move toward world prices, and the long-run necessity of a two-price system or of a vast surplus-disposal program abroad would diminish. There would be, of course, the problem of orderly (and non-disruptive) liquidation of existing stocks and of possible lags in movement of resources out of wheat. Neverthe-

less, from the vantage point of effects upon world trade in wheat, relations with other wheat-producing nations, and a market oriented world economy, Professor Farnsworth's program would have much merit. That it entails economic, political, and sociological problems is also evident.

Wheat, as a spectacular case in point, may illustrate that at last our left hand must come to terms with our right hand. The deep-rooted, persistent conflict between U.S. farm policy and U.S. foreign economic policy—indeed U.S. foreign policy generally—has been subsurface and on occasion fortuitously muted, but it remains to be resolved. Perhaps a necessary preface to such a resolution is our full realization that part of the high price of being a great economic and political power is that the area within which we can comfortably and unilaterally make policy for, by, and of ourselves alone is shrinking fast.

PROBLEMS OF ECONOMIC INSTABILITY IN OTHER COUNTRIES

STABILITY PROBLEMS IN THE SCANDINAVIAN COUNTRIES DURING THE POSTWAR PERIOD

By Erik Lundberg
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Introduction

Comparative studies of economic issues and economic developments in different countries must always be a difficult and dangerous kind of activity—at the same time it may be most stimulating for fresh thinking on old problems. One of the dangers in this case is that the author has not a well-balanced knowledge of the countries in question. He has a rather intimate knowledge of the ins and outs of Swedish policy measures, of the reasons for the measures taken, the relevant discussions on various levels of sophistication, and also of the poorness of available economic statistics that are relevant. This kind of knowledge is an irritating reminder of a very insufficient insight into the corresponding Danish, Norwegian, and Finnish problems. The outward manifestations of instability showing up in time-series statistics and in records of economic discussion often give very insecure ground for an understanding of the policy issues. It is from this point of view that I must be pardoned for putting an emphasis on Swedish experiences out of proportion to the weight of Sweden. The evidence of the other Scandinavian countries is mostly used in order to show interesting deviations from the Swedish pattern.

Stability Problems

The Recessions. As in most other West European countries, the curves of real national product and total industrial production give a strong impression of "balanced or even growth" with small interruptions. As to real gross national income, these interruptions tend to take the form of a slowing up of growth rather than absolute general declines.

The recessions are more noticeable in the series for industrial production. It should be observed that the dating of turns in the cycle is a dangerous game, not least for the Scandinavian countries, where seasonal fluctuations are strong and varying. There was no recession in 1948-49; only some lowering of inflationary pressure was noticeable. The recession after the Korean boom came first to Denmark, when

looking at industrial production in the first quarter of 1951, reaching a trough in the third quarter of 1952. This recession came about half a year later to Norway and Sweden and still later to Finland. The order of size of the production decline varied around 2-4 per cent. Industrial production showed weakness again around the second quarter of 1957 in all four Scandinavian countries. The relative depth of this recession in industrial production seems to have been of quite varying importance. In Finland industrial production declined by nearly 10 per cent in the first half of 1958 (but on an annual basis the decline from 1957 to 1958 was only 4 per cent). Also, in Norway the contraction in industrial production was regarded as a serious setback. In Sweden and, especially Denmark, the recession was mild and short. Comparing the years 1957 and 1958, Sweden's and Denmark's industrial production increased by 2-4 per cent.

One very general problem, then, is to what extent this absence of serious setbacks in total production and employment is due to relatively high built-in stability in these economies, to relatively weak external impulses from outside, or to successful stabilization policies. The term "relatively" refers both to experiences during earlier periods and to comparisons with other countries, especially the U.S.A., during the postwar period. Here I shall only give some very tentative remarks on this problem.

External disturbances mainly coming from the side of exports (volume and prices) seem to have been relatively small during the postwar period. The declines in total export value from the peak year to the recession year amounted to about 4 per cent of GNP in Norway, 1-2 per cent in Sweden, and 3 per cent in Finland. (Denmark did not experience any decline in export value during the postwar recessions.) In the case of Sweden, the postwar experience may be compared with an export disturbance about five times as large during the 1929-32 depression.

The sensitivity of an economy to an export disturbance (in volume terms) may be illustrated in various ways. As a very crude measure, we may compare percentage changes in GNP (in fixed prices) and in industrial production with simultaneous changes in export volume over the same periods. For Sweden the results are as follows (the experiences seem very similar in the other Scandinavian countries).

PERCENTAGE CHANGE

	1929–33	1951–52	1957–58
Export volume	−32	−11	−1
Industrial production	−11	− 2	+2
Real GNP	−10	+ 2	+1

Changes in investment can to some extent also be regarded as autonomous and as "external disturbances," but partly, especially with regard to inventories, these changes are a part of the "sensitivity problem." It is characteristic of the reaction pattern during these mild postwar recessions (never lasting much more than a year) that there hardly was any time for fixed investment to react. Large backlogs of orders, accumulated during the previous boom, mean that investment in machinery usually will continue to rise during the recession. The same tends to be the case with building activity. It is characteristic that investment in Norwegian shipping, being a branch of activity with high cyclical sensitivity, continued to rise rapidly—and especially rapidly—during 1958. Adding effects of government investment expansion, the result is that for Norway, Denmark, and Sweden the volume of total fixed investment has been stable or increased during the postwar recessions.

One main element of instability refers—as is generally the case during short cycles—to changes in inventories. In Sweden and Denmark the shift from rapid accumulation of stocks in 1957 to some decumulation in 1958 corresponded to about 2-3 per cent of GNP. It should be noticed, however, that such a figure does not—as it probably does in the U.S.A.—represent a corresponding instability effect on the internal economy. A large part of the change in inventories during the business cycles in these countries refers to imported products, and the effects are therefore transmitted to other countries. It should also be observed that the gross aggregate of investment in inventories contains sizable contracyclical movements of stocks of export products (especially wood products) that imply a dampening of the effects on production and employment of the cyclical changes in exports.

Inflationary Influences. The inflationary forces have been strong during most of the postwar period in all the four Scandinavian countries. I cannot go deeply into the inflation issue as a separate problem of instability. But certainly inflationary tendencies have had effects on cyclical instability, at the same time that anti-inflationary policy measures taken have been influenced by attitudes toward inflation. Instability in price and income formation has been an important problem in all the Scandinavian countries.

There are significant differences in this respect among the Scandinavian countries: Finland has had a rate of general price rise (+9 per cent per year for the cost-of-living index, 1947-57), being more than twice as high as in the other Scandinavian countries. The problem under this heading is how a varying degree of inflationary pressure has influenced cyclical behavior as well as the policy measures that have been undertaken. It should be observed that the expansion period 1958-60 was unique for all the four countries, as there was no, or very little, general rise of prices.

If there is—as was the case during most of the period 1945-57 in
Sweden and Norway—an excess of total demand in the goods and labor
markets at given prices and wage rates, then general recessions arising,
e.g., from a decline in export demand may not readily occur. But the
excess demand pressure during a boom is always very unevenly dis-
tributed and may be inflated by expectations that are dependent on the
very same boom conditions and therefore unstable. This instability ele-
ment refers in a high degree to inventory demand. A shift from inventory
accumulation to decumulation played an important role in the Korean
recession (inventory cycles have been marked in such important Scandi-
navian branches of industry as textiles, wood industries, iron and steel,
and iron ore). It should be added that the Scandinavian economies,
operating close to full employment (especially Norway and Sweden),
tend to move relatively easily into a boom with excess demand condi-
tions, which tend to concentrate on investment sectors.

When we come to discuss the special effects of a cost and price
inflation on cyclical stability, it must first be said that on this question
we have very little in the way of secure knowledge but a good deal of
wishful suppositions. It is, however, likely that the more or less steady
rise in prices and wages, having proceeded during two decades, has
created expectations of a future long-term trend of the same kind (this
view has been prevalent among Swedish businessmen, at least up to
1959). This factor may help to explain the strong growth trend of
investment in the private sectors and the fact that there has been very
little decline in private fixed investment during recessions, as well as
the difficulty in keeping these investments within bounds during times
of expansion and high-level employment. It should be added that the
repeated price and wage explosions during the postwar period—under
certain kinds of economic policy—may have a depressive effect on eco-
nomic activity. In Finland we can study the clearest cases of how
rising unemployment and stagnating production may result from a too
rapid increase of prices and costs (especially in 1956 and after the de-
valuation in 1957) under very restrictive fiscal and credit policies.

"Inflation sensitivity" of the Scandinavian economies to impulses
from a rise in export and import prices and from excess demand for labor
seem to be high. With respect to the first kind of impulse, the Finnish
and Swedish wood-industry sectors especially seem to function as an
effective channel for price-wage increases. The Korean boom offers an
outstanding example of this sort of sensitivity. The highly organized
pressure groups imply an efficient transmission of expansionary im-
pulses throughout the economy, and of course there is no reversibility
as to wage costs.

Unemployment Variations. The fact that postwar cyclical movements
of total production and employment in the Scandinavian countries have

been small does not exclude large swings in first differences. Rates of unemployment show very large relative changes as between boom and recession years in all four Scandinavian countries, varying between a minimum of 8 per cent (1954) and 12.5 per cent (1952) for Denmark, 0.9 per cent (1950) and 2.3 per cent (1958) for Norway, 1.5 per cent (1956) and 2.8 per cent (1953) for Sweden. These variations must be interpreted from the point of view of national full employment goals. These goals seem to have been higher in Norway and Sweden than in Denmark and Finland, and these differences have affected policy decisions. From this point of view the employment policy carried out in Sweden during recent years is of special interest and will be discussed below.

It should be observed that the levels of unemployment rates for different countries are not comparable. The Danish rates that seem very high refer to workers in voluntary insurance schemes—and therefore contain groups with relatively high unemployment risks. The Norwegian figures refer to an obligatory insurance system. If the figures of Danish employment were related to total number of wage earners, the unemployment rate would be around 4 per cent in 1954.[1]

The cyclical instability of employment to a very large extent refers to "islands of unemployment," consisting of special branches of activity, special regions of the country, and special groups of workers. The kind of problem much discussed in the Scandinavian countries refers to the issue that total demand may be high enough even in mild recessions but not effective with reference to these islands of unemployment. Additional total demand pressure may imply extra inflationary push in boom sectors of the economy, raising profits and accelerating wage-drift tendencies without affecting unemployment in the "islands" very much, at least not in the short run.

Balance-of-Payments Difficulties. The small variations of activity during the cycle refer with special importance to changes in the foreign exchange position of the various countries. It is self-evident that all the small Scandinavian countries are very dependent on foreign trade. As a percentage of gross national income, the value of exports of goods and services varied around 25 per cent for Finland and Sweden and was close to 40 per cent for Denmark and Norway.

The short-term instability of the Danish economy has to a large extent been determined by a series of exchange crises: in 1947, 1950-51, 1954-55, and 1957. Of the other Scandinavian countries, Finland has been most exposed to foreign exchange troubles with a number of devaluations in 1945, 1949, and 1957. Sweden and Norway have had

[1] For this computation, see, E. Hoffmeyer, "Stabile Priser og Fuld Beskaeftigelse" (Kobenhavn, 1960), pp. 68-73.

more potential exchange difficulties than actual crises. In all these countries, current or expected exchange problems have, however, been important determinants of short-run economic policy, although in varying degrees and in different ways.

In Denmark and Norway, the balance-of-payments situation has to a large extent been influenced by sharp variations in terms of trade. Denmark's terms of trade may move against her in depressions (as in the thirties) as well as during booms, when prices of imported raw materials rise more rapidly than export prices of agricultural products (representing about 50 per cent of total value of exports) and manufactured goods. In fact Denmark's terms of trade deteriorated by more than 20 per cent from 1949 to 1951, while Norway's and Sweden's terms of trade during the same time improved by nearly as much. In Norway it is freight rates (shipping providing 50 per cent of total export receipts) that determine the big amplitude of terms-of-trade fluctuations. Sweden is lucky enough to have raw materials and finished products in about the same proportions on both sides of the balance of payments, so that the amplitude of terms-of-trade fluctuations normally is of much less importance to the economy than in the other Scandinavian countries.

Exchange crises or exchange difficulties in these countries are of course not only determined by terms-of-trade fluctuations. In Sweden relatively high costs of production (at given exchange rates) have with great persistence been supposed to imply coming exchange difficulties—and this has had an influence on wage policy and government economic policy issues. Effects of high production costs on the balance of payments cannot be studied in isolation from the influence of home demand. Too rapidly increasing home demand has in all these countries at times been connected with foreign exchange problems. Denmark gives the clearest case of this type of instability. During the postwar period, as well as in the thirties, the instability of Denmark's economic development has mainly resulted from her vulnerable balance-of-payments position. With very low reserves during the postwar period up to 1958, changes in export or import values corresponding to only 1-2 per cent of GNP could imply an exchange crisis. Restrictive monetary and fiscal policy measures were introduced in 1951, 1954, and 1957 because of exchange difficulties. The short-term result was that production and especially building activity were cut down and unemployment created. This occurred at a very early stage of the 1951-52 recession and helped to nip the 1954 expansion in the bud. After 1957, Denmark has for the first time during the postwar period experienced rapid economic development without exchange reserve restrictions.

Economic Structure and Specific Cycles. The economic structures of

the countries in question are quite different, accounting for differences in cyclical experiences. From the point of view of short-term fluctuations in activity, there are certain branches of industry that show specific patterns and have special importance for some of the countries. The most evident cases are shipping for Norway, agriculture for Denmark, and forest industries for Finland and Sweden.

Shipping for Norway—yielding nearly 50 per cent of gross export income and representing 30 per cent of gross investment—shows specific cycles of its own. The interesting observation about this most important branch of industry for Norway is that the heavy fluctuations in freight rates and in gross income from shipping as well as in investment in new ships have had surprisingly small effects on the internal economy of Norway. The fact that the sharp fluctuations of the shipping industry are not effectively transmitted to the Norwegian economy is mainly explained by the following considerations. Investment in ships is to a dominating extent carried out by foreign shipyards and financed by foreign credits as well as by internal profits of the shipping companies. When, as in 1958, sharply falling freight incomes were combined with record investment in new ships, a potential foreign exchange crisis was prevented by long- and short-term credits from abroad, so that exchange reserves actually increased during the year.

The wood industries (sawmills, paper and pulp factories, etc.) generally show high cyclical sensitivity, but with great differences as between the sectors. With the raw materials produced internally, nearly the whole impact of cyclical changes is kept at home—in contrast to the case of shipping in Norway. It should be mentioned in this connection that in the case of Sweden the recessions after the Korean boom of 1950-51 was to a large extent determined by a succession of setbacks and revivals of individual industries. This lack of synchronization—the timing of turning points stretched out from 1951-52 for textile and forest industries to 1953-54 for engineering and iron ore—implied an evening out of total effects, at least in a statistical sense. Partly this result may be attributed to the unusual cyclical pattern in the U.S.A. where there was a partial recession in 1951-52 in the civilian sectors of the economy, more than compensated by the rearmament boom up to 1953, followed by the recession of 1953-54. This kind of lack of synchronization of activity changes in various branches of Swedish industry may also be regarded, however, as a consequence of the relatively low amplitude of aggregate fluctuations mentioned above. The cumulative forces, pulling down production and employment over large sectors of the economy, have not been strong during the postwar period. Uneven development of production and employment has, on the other

hand, raised important economic policy problems of another kind than
those pertaining to general fluctuations of total demand of the interwar
type.

Economic Policy Measures and Instability. The cyclical behavior of
production, employment, etc., in the various countries is in a varying
degree a function of economic policy. It is in general impossible to
isolate government (including central bank) activities and speak of
inherent cyclical characteristics of the rest of the economy or of the
private sector of the economy. In the account of the various features of
instability so far presented, I have repeatedly had to refer to changes
in economic policy. The government sector may have a special cyclical
response pattern interrelated with economic changes in the rest of the
economy—and observation of this special pattern of behavior, which
we may sometimes call stabilization policy, must be a part of a study of
the business cycle experiences of a country.

In such a study it is interesting to observe and analyze how the coun-
tries in question differ in applying the instruments of economic policy.
Denmark and Finland have kept a high level of interest rates and have
relied heavily on monetary policy. At the other extreme, the govern-
ment authorities in Norway have kept a low level of interest rates a
longer time than the other countries and have preferred to use physical
controls and fiscal policy measures more or less on the basis of na-
tional budget plans. Sweden has taken a middle course, experimenting
with considerable variety of measures. But timing and patterns of
policy measures have been transformed over time in each country, with
changing attitudes to changing economic situations, and any summary
characterization of this sort must be of doubtful value.

In an interpretation of the varying policy behavior, it must be borne
in mind that the political situations have been very different in the
Scandinavian countries during the postwar years. Denmark and Finland
have had relatively weak and shifting governments, while Norway and
Sweden have had strong socialist governments. I cannot here discuss
the importance of "the political possibility surface" for the measures
applied in a given situation. I should just like to mention the possibility
that the relatively greater importance of monetary policy in Denmark
and Finland may to some extent be explained by strong central bank
positions in relation to the usually weak and shifting governments in
these countries.

An ambitious full employment policy may during a recession imply
such expansion of various forms of public investment and consumption
that there will be little space left when a strong revival of demand
occurs within the private sector. Expansionary fiscal policy during

recessions will tend to create a more rapid growth trend of public expenditure than otherwise would be the case. Swedish national income statistics 1946-57 may be used to demonstrate this point.

I should also mention a problem that is often discussed: Do economic policy measures tend to create certain forms of instability? The clearest case may be the Norwegian building regulations that during the period 1946-56 apparently created a special kind of building cycle. The government tended to grant building licenses in a two-year cycle with peaks in election years, when licenses were emitted much in excess of building capacity and the available supply of building materials. The consequent rise and fall of building starts gave rise to a complicated pattern of variation in building activity and in the period of production of houses.[2]

Credit restriction measures applied in Denmark and Finland because of apprehensions of actual and expected foreign exchange difficulties or because of too rapidly increasing prices and costs seem to have had dampening effects on growth, and sometimes unemployment increased before the international turning point. This kind of creation of instability was in a way intended in order to lower the risk of a balance-of-payments crisis. Economic policy aiming at general stability may and will, as a rule, as already mentioned, create instabilities within sectors of the economy. This kind of problem has especially arisen in the case of building activity and to some extent also in industrial investment. Higher interest rates and credit restrictions have tended to cut down house building during inflationary booms, and then in periods of rising unemployment measures have been taken to stimulate house building. This kind of instability in building activity, accepted as a price for achieving more of general stability, is a more or less common experience in the Scandinavian countries.

Effects of Investment and Employment Policy: The Case of Sweden

It is evident from the summary account given above that stabilization policy in the Scandinavian countries has not been very successful. There have been repeated bursts of inflation, periods of overfull employment, loss of production both in booms and recessions, and at times too much unemployment, as well as uneven or unstable developments within sectors of the economy. Sudden changes in policy parameters (indirect and direct taxes, credit availability, subsidies) have created unstable conditions within sectors of the private economy. On the other hand, results may look rather good in relation to the interwar period. But of course these results must be judged with reference to the relative

[2] See *Planning in Norway 1947-1956*, by Petter Jakob Bjerve (Amsterdam, 1959).

strength of external disturbances and the cyclical sensitivity of the economies.

Let me offer some concluding remarks on a few interrelated instability problems, important to all the Scandinavian countries but having special bearing on economic policy experiences in Sweden during the period 1955-59. I think that a close study of economic policy measures applied during these years in Sweden can shed some light on the possibility of achieving successful stabilization results, as well as on the limitations and perhaps also the dangers of a very efficient policy.

It is not possible to say to what extent the expansion in total demand during 1957-59 was a result of the economic policy pursued by the Swedish government. A number of measures were adopted: an investment tax which during 1955-57 had been levied on expenditure on machines, construction, etc., was removed at the beginning of 1958; the credit policy was eased and the interest level lowered; building controls were relaxed and house construction increased; while at the same time government expenditure expanded strongly—with a record total budget deficit as a result. The investment incentive during 1958-59 was effectively reinforced by means of a special device of using investment funds reserves. During the period May, 1958, to September, 1959, a considerable number of firms were granted permission to use investment reserves for writing down investment expenditures incurred. The National Labor Market Board authorized the use of a large amount of such reserves, this being bound up with the condition that the investments in question should be undertaken rapidly. There was in fact very great interest in utilizing these possibilities of rapid writing down of investments combined with an extra allowance, but it is unfortunately impossible to judge how effective this incentive to investment actually was in comparison with the other policy changes.

The results of this expansionary policy must be studied in the light of the restrictive policy that was pursued during the boom of 1955-57. The period 1957-59 cannot be considered in isolation. A positive feature of the economic policy pursued during the boom of 1955-57 was that this policy was not so restrictive as to check the rate of economic growth, as the case appears to have been in some other countries during this period. The total volume of production rose at a record rate during the boom years of 1955-57, corresponding to approximately 4 per cent a year. However, the investment tax and the very restrictive credit policy, which was introduced in 1955 by means of raising the level of interest rates and using various forms of credit control, seem to have had considerable dampening effects, especially on industrial investment. Investment expenditure by industrial corporations on machines and

buildings remained at a fairly constant level in real terms during the boom and experienced a powerful increase first during the recession in 1958-59 (by about 15 per cent). In other words, the stabilization policy, over the full course of the business cycle, had been so successful in this sphere that the increase in investment expenditure was shifted from the upward to the downward phase of the cycle, thereby helping to eliminate the cycle. Surveys carried out among a great number of industrial enterprises during 1955 and 1956 seem to confirm the hypothesis that the combination of investment tax and credit restrictions caused a heavy reduction of investments below the *ex ante* plans. When the investment tax was abolished at the beginning of 1958, reinforced by the positive stimulus of the investment allowances and relaxation of credit policy and building controls, the cumulative consequence was that investment plans, which had been postponed on account of financing difficulties or excessive costs, were realized during 1958 and 1959. The total result of the investment policy, including an expansion of government investment and house building, was a rise in the volume of all investment expenditure (excluding stock investment) of 6 per cent to 1958 and as much again to 1959.

Three types of criticism may be directed against the type of stabilization policy carried out in Sweden during the period 1955-59, and I think that the points of criticism to be mentioned here very well can be applied to similar experience in the other Scandinavian countries: (1) Very little attention is paid to the need to isolate the results of the various parameter changes. So many significant policy changes are introduced over a short period of time that it will forever be impossible to read off the relative effectiveness of changes in taxes, rates of interest, credit controls, etc. Studies made during 1955 and 1956 among a number of industrial corporations of the effects on investment plans of the separate measures introduced during 1955 give interesting answers to the questions, but the validity of the replies can very much be doubted. (2) A serious criticism of the type of economic policy that has been pursued during the years 1955-59 refers to the observation that during both the upswing and the downswing, it has involved a combination of inflationary pressure and controls of the credit and capital market. During the years 1955-57, drastic credit restrictions were resorted to in order to hold back the demand for savings from the private business sector and to make room for the rapidly expanding central and local government investments, as well as house construction. "Savings did not suffice," however, and the excess demand partly found expression in a price inflation. During the following recession investment plans expanded, as has been illustrated above, within both the public and the

private sectors. A "lack of long-term savings" was reflected in difficulties in financing the government budget deficit, resulting in an extensive credit inflation and a big increase in liquidity. And this state of too much liquidity has resulted in new needs to control the development of bank credit during the 1959-60 boom. (3) One can look upon the retardation of production growth during 1958 as being a consequence of a certain lack of effectiveness in economic policy. The decline in output that was experienced in various sectors dependent on exports (certain forest products, iron and steel, iron ore) had not been counteracted sufficiently by greater production in housing and other construction activity. Reduced demand for exports had been partially offset by heightened public and private investment and consumption demand—but this offsetting action had not been so large as to eliminate a "demand deficit" corresponding to about 2 per cent of the national product. It was principally a lack of uniformity in economic development, with local unemployment not matched by increased local employment openings, that created problems. A flexible employment policy—which made use of various kinds of public works and measures in order to retrain workers and increase the mobility of labor—has in fact been an increasing element in the economic policy of the Swedish government.

From the point of view of a more ambitious full employment policy, it has been maintained by prominent socialist economists that a more selective economic policy than that carried out during 1955-59 could have achieved better stability results. A selective policy would imply government expenditure and credit injections directed at the islands of low activity combined with a restrictive general economy policy. This kind of stabilization policy, supplemented by attempts to increase the mobility of labor, has to some considerable extent been carried out in all the Scandinavian countries during recent years. But the Swedish government seems to have been most ambitious and successful along such lines. The issue is how far and with what degree of intensity such a policy should be applied in the future. There are obviously other fundamental political issues behind this kind of selective economic policy than just the stabilization targets discussed here.

GROWTH AND STABILITY IN THE
POSTWAR ITALIAN ECONOMY

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I wish to consider four questions: (1) the comparative postwar performances of the Italian and American economies; (2) transmission effects in Italy originating from American economic movements; (3) problems of growth and structure in Italy; and (4) conclusions suggested by over-all comparison of the two economies.*

I. *Comparative Behavior with the United States, 1947-59*

Compared with the United States since 1947, Italy has done far better in steadiness and scale of increase in total output, output per head, and industrial production. In fact, her rates of gain rank among the highest in the world. Real GNP has risen at an average annual rate of 5.5 per cent, or 5.0 per cent per head. Industrial production alone has averaged an astounding 10.2 per cent yearly increase.

Furthermore, no downturn has occurred in any single year. The closest thing to a "fluctuation" has been a fall in the annual rate of increase. Even here for real GNP the variance has been small. Only in 1952 did the gain fall well below average—2 versus 5.5 per cent— while the ratio of the standard deviation to the mean for the period ran at only .246 or .276, depending upon the series. For industrial production, 1949 was a deviant year on the high side, when the enormous increase of 27.4 per cent was recorded, while low gains were achieved in 1952 (2.9 per cent) and in 1958 (3.1 per cent). However, the variance was greater, with the ratio of the standard deviation to the mean standing at .607.

By contrast, the United States has experienced much smaller rates of gain in output and much sharper fluctuations. During 1947-59, real GNP rose at an average annual rate of 3.6 per cent, and at about 1 ⅝ per cent per head. However, during the recessions of 1949, 1954, and 1958, output actually fell. The range of annual rates of change was quite high, with the ratio of the standard deviation to the mean at .97.

Industrial production (1957 = 100) rose at an average rate of only 4.3 per cent, with sharp declines recorded in 1949 (−5.9 per cent), 1954 (−6.6 per cent), and 1958 (−7.0 per cent).

On the output side, then, the behavior of the two countries stands in rather sharp contrast. For the postwar years, Italy has had a very strong growth economy, while the American has proved to be recession-prone, functioning at relatively low pressure much of the time. Yet the postwar American record conforms well on average to Goldsmith's 1839-1959 trend values for annual increases in real GNP, total and per head. By comparison, the Italian data after 1946 diverge most sharply from their behavior during 1861-1940. Rates of gain postwar have been far greater, with complete absence of any downturns whatever in total production.

Turning, next, to prices, the implicit price deflators for GNP in both countries reveal a comparable and sustained upward creep since 1947, averaging 2.6 per cent a year in the United States and 3.2 per cent in Italy. In both countries large jumps occurred in 1948 and 1951: 6.6 and 7.6 per cent, respectively, for the United States and 11.4 and 9.0 per cent for Italy. The surge in 1948 is mainly attributable in both cases to adjustment to a greatly increased postwar money supply after decontrol of prices, while 1951 arose from extensive inventory speculation, coupled in Italy to a severe rise in import prices. Excepting these years, however, the annual rates of advance in the price level have been quite moderate, with Italy falling behind during 1956-59.

At the wholesale level, price behavior in the two countries has diverged markedly. In the United States, the average annual postwar rise was 2.0 per cent, while Italy actually recorded an average decline of 0.4 per cent yearly. This fall is somewhat surprising, given that wages generally have consistently risen rapidly, by at least 5 per cent yearly in industry, and that the imputed price change for final product advanced 3.2 per cent annually. The explanation is twofold: an enormous rate of gain in man-hour output of industrial workers, perhaps attaining 10 per cent yearly; and increasing foreign competition with domestic industrial products under the common-market program.

Although price levels in both countries have crept rather than jumped upward during most of the period, the behavior of private money supply—currency plus demand deposits in the hands of the public—shows a notable difference. In the United States, the average annual rate of increase was only 2.0 per cent after 1947, falling after 1952 to only 1.9 per cent. In Italy, the average yearly rise was 14.3 per cent, not falling below 6.5 per cent in any given year. Monetary restraint in the United States has become increasingly severe, in my judgment, checking the growth of output, but failing to create enough unemployment and idle capacity to terminate a creeping rise in unit costs and prices.

In the fall of 1947, Italy, too, introduced monetary restraint, imposing tighter reserve requirements and for a time requiring the government to finance its deficits by borrowing from savings. These measures broke the back of the severe first postwar inflation, although the government in recent years has relaxed its policy, using reserves held in the central bank to finance some of the increase in its debt. Even today, however, stability of the lira is still official gospel, leading to the dictum that attempts should not be made to cure the grievous unemployment problem by accelerating the rise in aggregate demand. This policy rests on the sound principle that capital shortage rather than investment-deficiency underlies the unemployment problem. Consequently, interest rates remain relatively very high, although money supply has risen very rapidly. Partly, this rise derives from use of reserves to finance government borrowing. Partly, too, it arises from a net inflow of 2.3 billion dollars in official holdings of gold and foreign exchange since 1954, which has fixed firmly the external value of the lira, at the same time strengthening domestic confidence in its stability.

Jointly, the failure of monetary turnover to advance pro rata with money supply and the rapid and well-sustained rise in output have proved decisive in preventing the inflationary creep from becoming a gallop, in much the same way as the late Sumner Slichter argued in behalf of more liberal monetary expansion for the United States. For Italy, more moderate restraint of effective demand has so far paid off handsomely, while severe restraint in the United States may have slowed both growth and improvements in productivity, making the economy more recession-prone.

Despite the remarkable Italian achievement on the output side, the comparative record is much less favorable for employment and unemployment. Yet superficially regarded, the demographic facts would lead us to expect a far better performance.

Resident population in Italy, which includes temporary emigrants, has been rising quite slowly since 1947—only 0.8 per cent annually, as against 1.7 per cent for the United States. Rough estimates from admittedly deficient data indicate that currently the Italian labor force is increasing only at 0.6 per cent yearly. For the United States, the comparable figure is about 1.9 per cent. Overlooking wide regional differences, the over-all Italian figures suggest the complete absence of population pressure, despite a widely held international belief to the contrary. There is even less basis for the notion of overpopulation. Per capita output and consumption have been rising at trend rates of over 1 per cent annually ever since 1885, and much faster since World War II. The Italian economy has proved both resilient and capable of sustained growth.

Nevertheless, massive unemployment has plagued the country

throughout the postwar years. Although the registered total probably understates the facts and in any case fails to express with any adequacy the equally crucial problem of underemployment, still it shows that unemployment has ranged between 1.87 and 2.20 million throughout. For a labor force of roughly 21 million in 1959, the unemployment rate was about 9 per cent, running even higher in prior years. Even against recession peaks of 5-7 per cent in the United States, the Italian record is obviously much worse—all the more so because these very high rates have prevailed consistently throughout the postwar years. Prior to a drop of 300,000 in registered unemployment during 1957-59, neither a trend nor a cycle is apparent from the data.

Figures for employment first became available in 1953, and these are only rough official estimates on an annual basis. Allowing for net reductions of 60,000 per year in agriculture, they suggest that over-all employment has been increasing about 225,000 yearly—by coincidence rather than design about reaching parity with the figure called for by the Vanoni Plan of 1955. More important, probably 80 per cent of this advance has occurred outside the industrial and construction sectors, centering in trade, services, and government. In sharp contrast, the running manual-worker sample of the Ministry of Labor shows a net increase of industrial employment of less than 3 per cent for the whole of 1948-58.

The failure of industry to expand employment, despite a doubling of its output in the same period, has high importance. Here is technically the most progressive sector of the Italian economy, where productivity and wages are high enough relative to levels elsewhere to justify our designating the whole system as dual in character. Yet employment in industry has shown little tendency to grow, while in the softer zones of the nonagricultural sector, where wages and productivity are generally far lower, employment has been rising quite rapidly.

Moreover, as the over-all figures clearly show, a persistently high rate of increase in national output is quite compatible with continuing mass unemployment. The explanation lies in a perverse combination of capital shortage, extreme industrial wage pressure, and a pronounced warp in favor of capital-intensive production and not in the more familiar Anglo-American case of oversaving and underinvestment.

II. *Interrelations and Transmission of Effects between the Italian and American Economies*

Italy emerged from World War II with perhaps a third of her fixed capital destroyed, badly depleted stocks, and a rate of production that by 1945 had fallen catastrophically to the level of 1900. Foreign aid, about 80 per cent American, saved the country from mass hunger and probably communism. It was also decisive in providing a new start,

initiating an amazingly strong recovery of output that put the economy
back on trend by 1951. By 1953, the temporary dislocation wrought
upon the balance of payments by the Korean war had been overcome.
Since then it has shown consistent and remarkable improvement, yield-
ing a large cumulative increase of official holdings of gold and foreign
exchange and permitting substantial liberalization of trade and ex-
change policies. In fact, during 1958 and 1959 the goods and services
balance turned positive for the first time in decades.

The principal reasons for the improvement in the Italian foreign
balance were rapid increases in tourism, earnings of nationals abroad,
emigrant remittances, and foreign investment in Italy. Throughout the
period since 1947, exports have climbed steadily. However, the poor
resource endowment of the country makes it extremely import-depend-
ent. Imports of raw materials, machinery, and equipment normally
make up two-thirds of the total by value. Thus the rapid advance of
production has kept imports rising consistently with exports, imposing
a persistent import surplus that traditionally has been financed by
tourism, services, and private remittances. Accordingly, the balance of
payments is very vulnerable to major changes in the world economy. A
sharp rise in prices of raw materials or a severe depression in the
United States and Western Europe would quickly create difficulties for
Italy.

By contrast, the easing of trade barriers with the Continent under
the common market has not led to problems for the foreign balance,
despite some expectations to this effect. Quite the contrary. Exports
have continued to rise, and Italian industry has proved quite capable
of increasing its efficiency.

So far, any disturbances transmitted to Italy from the United States
have been minor. Total exports show no decline in any of the Ameri-
can recession years—1949, 1954, or 1958. Figures available only from
1953 reveal that only in 1954 was there a drop in exports to the United
States. Total earnings from tourism have risen without break since
1947. Small declines occurred for private remittances in 1949 and
1954, but these counted for little in the over-all balance. Moreover, in-
flow of long-term investments into Italy, much of it American, has
soared sevenfold and without interruption between 1947 and 1958, at-
taining a sizable total of 182 million dollars in the latter year. Even
the sharp decline in foreign aid—from 403.7 million dollars in 1949 to
66.9 million by 1958—has caused no major difficulties. True, starting
with 1952, American offshore procurement purchases have afforded
some cushion, but not much. Including them, the over-all drop in aid
plus procurement between 1949 and 1958 was 71.8 per cent, as against
83.4 per cent if aid alone is considered.

In the entire postwar period, only two transmission effects from the United States had substantial short-term impacts, one favorable and the other unfavorable. On the one side, massive economic aid during 1944-50 was decisive to recovery and to setting off continued rapid growth. On the other, the inflation of world prices evoked by the Korean war enforced a large increase in the Italian import surplus, throwing the goods and services accounts seriously out of balance. Compared to a deficit of only 54.3 million dollars in 1950, this figure soared to 621.5 million by 1952, an increase of over eleven fold. Fortunately, adjustment to the new level of world prices was prompt and free of deflationary internal effects.

Over the longer term, continued growth and relatively high levels of American business activity have undoubtedly contributed importantly to improvement in the Italian balance of payments and to sustained growth in the Italian economy. Here must be included the rapid expansion of tourism, emigrant remittances, foreign investment in Italy, and exports to the United States. Excluding exports for lack of data, these items collectively jumped from only 61.2 million dollars in 1947 to 869.1 million in 1958. Since they all primarily reflect private decisions and are largely American in origin, they may be attributed to generally favorable economic conditions in the United States.

III. *Problems of Growth and Structure in the Italian Economy*

The first questions to consider are why the postwar growth in output has been so steady, and whether it can be sustained.

To interpret the postwar experience correctly, one must remember that the high average growth rate reflects in part a truly impressive recovery from virtual economic collapse by 1945. This astounding rebound took real GNP from a low of 76.6 billion lire in that year (prices of 1938) to 182.3 billion in 1951, when recovery and reconstruction had about been completed. Underlying the revival were the vigor and initiative of the Italian people, freed at last from the yoke of fascism; timely arrival of massive American aid; and stabilization of the lira after 1947. Within this context, large returns could quickly be had from replenishment of inventories, and repair or replacement of worn-out plant and equipment. The whole expansion was also helped greatly by ample labor reserves and opportunity for the first time in years to introduce the latest technical methods.

During 1951-59, Italy has been in the phase of development rather than recovery and the growth rate has stayed high. No relapse has occurred; so there has been no opportunity to test the workings of any automatic stabilizers, although their existence may be doubted. In the postwar years, the accumulation of inventories turned passive only

twice: in 1948, when inflation was halted, and in 1952. In both years the change was too small to invoke deflation.

To account for continued growth after 1950, we must consider the behavior of investment. At no time in those years has there appeared any deficiency of investment opportunities. In fact, gross investment has risen from 19.8 per cent of current GNP in 1951 to 24.7 per cent in 1959, giving Italy one of the highest rates of saving in the world, about equal to that of the Soviet Union and well in excess of that in the United States. On a capital-to-output ratio of about 3:5 (gross investment to increment in output lagged one year), this high rate of investment has fostered a high rate of growth in output. Both have proved remarkably steady for the last several years.

Extensive government intervention did much to stabilize the growth rate at high levels. By tolerating rather rapid monetary expansion, even at the cost of some inflationary creep, government contributed a buoyancy to effective demand, also negating much of the cost pressure from rapidly rising wages. Government programs for the industrial development of the South and other backward regions have required large and continuing public investment, stimulating formerly stagnant markets, creating numerous external economies favorable to lower private costs, and opening up larger internal uses for domestically-produced capital goods.

On the investment side, housing and public works together—both largely government expenditures—have risen 171.6 per cent in current value between 1951 and 1959, while outlays on plant, equipment, and additional inventories have advanced only 54.7 per cent. In 1951, housing plus public works accounted for 26.9 per cent of current gross investment. By 1959, they had risen to 33.6 per cent, with public works absorbing about a quarter of the combined total for both together. Even if there had been an underinvestment tendency in the private sector—a fact that is impossible to establish and that may be strongly doubted—it was fully offset by substantial and increasing public investment throughout the period. Fill-in expenditure by government, as a deliberate discretionary act, has made sure that offsets to saving would at all times be adequate—perhaps even cutting into private investment at times. As had been proposed by the inoperative Vanoni Plan for overcoming mass unemployment and promoting development, government investment has served as a discretionary stabilizer for growth, capable of considerable adjustment to compensate for possible variations in private investment.

Moreover, the latter has proved buoyant to its own account. Partly it has been induced by the steady growth in effective demand. Partly, too, it reflects the high cost-savings made available from technical im-

provements and called forth by increasing foreign competition. Finally, it involves exploitation of newly discovered resources, such as oil and gas.

Will the postwar growth rate be sustained and the economy stay free of downturns? No one can say, but there is some reason for doubt. The behavior of real GNP between 1861 and 1940 reveals plainly that the Italian economy was then vulnerable both to major and minor cyclical fluctuations. If the past may serve as a guide, fluctuations will recur, although discretionary policies may curb them more effectively in the post-Keynesian world to come. As for the rate of growth, viewed against the past it is currently very high. For all of 1861-1956, the average annual advance was only 1.9 per cent, including or excluding the badly disturbed period of 1940-50. Around 1897, the trend of growth increased as industrialization began to take hold. For 1897-1939, the average rate of advance rose to 2.2 per cent, compared to only 0.8 per cent for 1861-97. Even limiting ourselves to the later period, the rate is about one-half that for the years after 1950.

Undoubtedly, the postwar Italian performance has been aided greatly by a well-sustained expansion throughout Western Europe and, to a lesser extent, in the United States. Any real slackening abroad would quickly cause deterioration in Italy as well, given her extreme dependence upon foreign trade and other external income. Furthermore, the present surge of investment in industry may partly reflect transitory access to an accumulated pool of technical improvements that eventually may become exhausted. Finally, some tightening of the labor reserve seems likely in the sixties as the number of unemployed is gradually worked down—a process already under way. As yet, however, slackening of the postwar growth rate is only a speculative possibility. The present may turn out to be merely a passing period of exhilaration. Or it may prove to be part of a shift to a new normal, such as occurred around 1897.

What can be affirmed with certitude is that there is a major difference of structure between the Italian and American economies. Italy does not yet have a unified industrial system. Consequently, her major economic problem is to effect such a transformation. All of her main difficulties—unemployment, underemployment, low productivity, poverty, and inflation—connect up with this transition and its successful completion.

The Italian economy possesses a dual economic structure in several respects: in technology, forms of enterprise organization, productivity, wages, and the employment of labor. The bifurcation is most evident in the contrast between North and South, but it is not solely a geographic problem. It also is visible within certain industries, within agriculture, and even within urban communities. On the one side are modern firms

using modern methods, employing wage labor at comparatively high
rates of pay. On the other are peasant proprietorships, artisan houses,
family retail shops, and an army of self-employed engaged in various
petty pursuits. Here prevail traditional techniques, using little capital
per man and yielding generally very low returns to labor.

Partly, this dualism in economic structure merely reflects incomplete
industrialization, dictated by limited availability of saving. Partly, too,
it arises from the failure of capitalistic methods to spread fully through-
out the country, for both political and cultural reasons. Beyond limited
trading relations, the centers of capital in the northern triangle simply
have never really extended themselves deeply into the South and the
Islands, which have largely remained export economies heavily domi-
nated by localized activities of a traditional sort.

Beyond these impediments, however, full-scale industrialization has
been further retarded postwar by a perverse combination of wage and
investment policies within the advanced sector of industry. Increasing
volume, massive investment, and extensive reorganization together have
generated great improvements in productivity and large cost savings.
For political reasons, these gains have been shared with the workers,
under the leadership of highly wage-conscious and competing unions. In
consequence, industrial wages have risen very rapidly. At the same time,
however, the rise of wages has imparted a strong capital-intensive bias
to investment in the sector, admitting that new techniques and exploita-
tion of new resources have worked independently toward the same end.
Both from the factor and the goods sides, capital-intensive production
has been strongly favored. The results have been twofold: little absorp-
tion of labor by the advanced sector and a widening gap in returns to
labor, favoring those fortunate enough to have jobs in that sector.

Accordingly, three of the foremost problems for development policy
are to check in some way the rapid rise of industrial wages, to translate
cost savings into lower prices for industrial goods, and to accelerate the
modernization of the remainder of the system, so that a more cohesive
and more efficient economy can gradually emerge.

IV. *Conclusions*

The Italian economy has grown much faster than the American since
1947, has had no recessions, and has improved the productivity of in-
dustrial labor much more rapidly. Here two influences have proved de-
cisive: an initial period of recovery and reconstruction and a continuing
problem of transition to full industrialism. Both have required massive
investment, both public and private. A high and increasing rate of sav-
ing has made such investment possible, without acute strain upon do-
mestic real resources. In fact, Italy has been able to avoid serious mone-

tary and fiscal restraint, electing to accept creeping inflation rather than slower growth and even heavier unemployment.

At the same time, Italian development policy since 1952 has caused no evident strain for the foreign balance. Indeed, the rapid rise of tourism, emigrant remittances, earnings of nationals abroad, and foreign investment in Italy together have greatly increased the amount of external resources available for domestic use. Holdings of gold and foreign exchange have grown rapidly. Finally, apart from the Korean disturbance, American fluctuations have had no significantly adverse impacts.

Compared to the United States, the Italian record is poorer only for unemployment and underemployment. However, these difficulties flow from a structural problem having no real counterpart in the United States. The very existence of a pressing need for over-all transformation imparts a peculiar stimulus to growth, although it also poses hard questions for wage and investment policy.

One swallow does not make a summer, but it may be ventured that easier money and fiscal policies have had something to do with the exceptional success of the postwar Italian economy, while their converse in the United States helps to explain our remarkably less impressive record.

GROWTH AND STABILITY OF THE POSTWAR JAPANESE ECONOMY

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Introduction

The postwar economy of Japan is characterized by an exceptionally high rate of growth, sustained thus far with little sign of retardation. Superficially, cyclical patterns are discernible; but they are in terms of changing values of positive rates of growth, even the smallest among them having been 3.7 per cent (in the year-to-year change of real gross national product). Looking behind the statistical indicators, furthermore, we find the impact on the economy of so many extraordinary and/or noneconomic factors that we become wary of applying the tools of cyclical analysis to our subject matter. Thus the focus of this paper, partly for the reason of the limitation of space, is on the how and why of the high rate of growth which the Japanese economy appears to be maintaining, at least up to the end of 1960.

Summary of Statistical Indicators

With a view to giving a broad outline of the process of economic growth (and/or fluctuations) in the postwar Japan, Table 1 below summarizes some of the relevant statistical indicators in annual series and Table 2 presents annual rates of change in a selected number of series.

A number of salient points may be gleaned from these two tables:

1. Real GNP grew steadily over the entire period with little sign of retardation in the rate of growth. The picture of growth may be summarized by comparing three-year averages at the three points of time, as follows:

1946–48	100		100
1952–54	171	100	
1958–60		160	274
Average annual cumulative rate of growth	9.2%	8.2%	8.6%

The average rate of growth of 8.2 per cent between 1952-54 and 1958-60 can be said to be truly remarkable.[1]

[1] Between 1926 and 1930, a period of extraordinary expansion in the prewar Japan, such a rate was 4.6 per cent.

TABLE 1

MAJOR ECONOMIC INDICATORS IN POSTWAR JAPAN

Year	GNP[a] (real) (Billions of Yen)	GNP per Capita[b] (real) (1934–36 =100)	Manufacturing Production Index[c] (1955 =100)	Crude Index of Productivity in Manufacturing[d] (1955 =100)	Commodity Exports[e] (Millions of Dollars)	Wholesale Price Index[f] (1934–36 =1)	GDCF[g] / GNP	ΔGNP[b] / Private Fixed Capital Investment	Special Procurement[i] Gross Receipts from Abroad on Goods and Services
1946	10.9	39	16.1		103	16.3	30.9%		
1947	13.0	68	20.1	22.6	174	48.2	32.6		
1948	13.9	71	26.9	33.2	258	127.9	33.7	91%	
1949	14.5	73	33.7	41.1	510	208.8	27.6	80	
1950	16.2	80	44.7	54.0	820	246.8	24.7	103	
1951	18.4	89	63.1	74.0	1,355	342.5	26.8	127	28.2%
1952	20.4	97	68.0	77.3	1,273	349.2	24.3	102	37.6
1953	21.7	102	84.2	91.7	1,275	351.6	25.8	70	36.9
1954	22.6	105	92.4	95.2	1,629	349.2	22.7	69	25.2
1955	24.9	114	100.0	100.0	2,011	343.0	25.1	75	20.8
1956	26.9	122	123.5	112.6	2,501	358.0	29.6	77	18.2
1957	28.8	130	146.4	119.4	2,858	368.8	30.9	80	15.1
1958	29.9	133	147.0	117.2	2,877	344.8	27.3	63	13.6
1959	35.0	154	185.0	136.4	3,457	348.3		67	11.5
1960	38.7	169							

[a] Estimated by Economic Planning Agency and published in *Kokumin Shotoku Hakusho*, 1960, p. 192. The figure for 1959 is preliminary and the one for 1960 is an estimate on the basis of the preliminary figures up to the end of Oct., 1960. The series is expressed in terms of 1934–36 average prices and is for fiscal years beginning in April.

[b] Based on the GNP figures in the table, divided by the population figures as of Oct. 1 for each year, and made into an index series by taking 1934–36 average as 100. Thus these are fiscal year figures also.

[c] Constructed by the Ministry of International Trade and Industry. See the Bank of Japan, *Economic Statistics of Japan, 1959*, p. 2.

[d] Obtained by dividing the index of manufacturing production in the table by the index, with the same base, of employment of regular workers in manufacturing. The latter index, constructed by the Ministry of Labor, is available for the years starting in 1951. (See *ibid.*, p. 289.) For the earlier years, the Labor Force Survey figures for manufacturing by the Ministry of Labor were linked to the index by the author. The procedure admittedly is not very satisfactory.

[e] Customs returns figures, published in *Tsusho Hakusho, 1960*, p. 568, for 1950–59, inclusive. For the earlier year figures, see Tsuru, *Essays on Japanese Economy*, p. 33. The single exchange rate of 360 yen to a dollar went into effect in Apr., 1949.

[f] The Bank of Japan linked series. See the Bank of Japan, *op. cit.*, p. 269.

[g] Estimated by Economic Planning Agency and published in *Kokumin Shotoku Hakusho*, 1960, p. 193. Fiscal year figures. Ratios are obtained by dividing the *real* gross domestic capital formation by the *real* GNP for each fiscal year.

[h] Both terms in the ratio are real (1934–36 average prices) fiscal year figures each smoothed by taking three-year moving averages. Ratios are obtained by lagging ΔGNP by one year. Original data are taken from *Kokumin Shotoku Hakusho*, 1960, p. 182, 192, and 198, except the figures for 1959 and 1960 are estimates by Osamu Shimomura in his paper read at the Meeting of Japan Theoretical Economic Association, Oct. 22, 1960, in Nagoya.

[i] "Special procurement" is understood here in a broad sense and its annual series is taken from *Tsusho Hakusho, 1960*, p. 578. "Gross receipts from abroad on goods and services" is the sum of "Customs returns figures of exports" (f.o.b.), "Gross receipts on services, other than special procurement," and "Special procurement." See *ibid.*, pp. 568, 574–75.

2. Index of manufacturing production also presents a roughly similar picture except the early postwar years are more patently characterized by the special process of recovery from the war-end nadir in industrial activities.

3. Per capita real GNP also grew steadily; but the prewar 1934-36 level was exceeded for the first time only in 1953.

4. The ratio of gross domestic capital formation to GNP fluctuated between 22.7 and 33.7 per cent and averaged 27.8 per cent over the years from 1946 to 1958, inclusive.

5. Excepting the three off-years of 1952, 1953, and 1958, commodity exports expanded generally at a remarkable rate. It has been estimated[2] that the elasticity of Japan's exports with respect to world income in

[2] By Dr. O. Shimomura. See his paper read at the meeting of the Japanese Asso. of Theoretical Econ., Oct. 22, 1960, in Nagoya, Japan.

TABLE 2

ANNUAL RATES OF CHANGE IN MAJOR ECONOMIC INDICATORS IN POSTWAR JAPAN[a]

Year	GNP (Real)	Manufacturing Production Index	Crude Index of Productivity in Manufacturing	Cash Earnings of Regular Workers in Manufacturing (Real)[b]	Commodity Exports
1946–47	19.2%	24.9%			68.7%
1947–48	7.4	33.8	46.9%	42.0%	48.2
1948–49	3.9	32.7	23.8	25.0	94.1
1949–50	12.2	25.2	31.4	28.3	60.7
1950–51	13.5	41.1	37.1	6.2	65.3
1951–52	10.5	7.8	4.5	11.2	− 6.1
1952–53	6.7	23.8	18.6	5.1	0.2
1953–54	3.9	9.7	3.8	− 0.9	27.8
1954–55	10.1	8.2	5.0	4.9	23.5
1955–56	8.2	23.5	12.6	8.9	24.4
1956–57	7.1	18.5	6.0	0.4	14.3
1957–58	3.7	0.4	− 1.8	3.2	0.7
1958–59	17.0	25.9	16.4	7.3	20.2
1959–60	10.6				

[a] Except the column on "cash earnings of regular workers in manufacturing," all the rates of changes are calculated from the figures in Table 1.

[b] The figures for 1951–52 to 1958–59, inclusive, are calculated from the statistics given in the Bank of Japan, *Economic Statistics of Japan, 1959*, p. 289. The figures for the earlier years were estimated by the author on the basis of statistics in Tsuru, *Business Cycles in Post-War Japan* (1953) and linked to the series for the fifties. It should be possible to improve upon the figures for the earlier years.

the recent years has been 3 to 3.5; in other words, when world income rose by 4 per cent, Japan's exports rose by 12 to 14 per cent.

6. The level of wholesale prices rose more or less steadily, at an inflationary pitch, until 1951; but since then it has remained fairly stable, the 1959 level being 1.7 per cent above that of 1951. Meanwhile, the unit value index of exports declined by 22 per cent over the same period.

7. The annual rate of growth of real GNP presents a cyclical pattern, registering the low point of less than 4 per cent in 1948-49, 1953-54, and 1957-58. At the latter two points, the wholesale price index also turned downward. The slump in exports appears to be associated, with a lead, with the decline in the rate of growth of real GNP.

Our problem is first to acquaint ourselves with certain historical background of the period and then to attempt an explanation for the pattern of growth summarized above.

Certain Historical Background

1. *Abnormalities.* Japan surrendered on August 15, 1945; and at least roughly for the subsequent four years her economy was in an extremely abnormal condition.[3] Abnormalities consisted of: (1) the process of re-

[3] See S. Tsuru, *Business Cycles in Postwar Japan* (Tokyo: Sci. Coun. of Japan, 1953).

habilitation from the nadir of defeat in a major war—the nadir characterized by the loss of one-fourth of the reproducible physical wealth and by the extreme slump in productive activities, less than 20 per cent in industrial production as a whole, and about 60 per cent in agricultural production, both compared with the 1934-36 averages; (2) the inflation of substantial magnitude, continuing unmitigated until the first quarter of 1949, with an accompanying phenomenon of widespread black and grey market prices in both consumers' and producers' goods; (3) the overriding influence of noneconomic policy decisions by the Occupation authorities on the economic life of the country—the policy decisions which were at first oriented towards restricting the tempo of recovery and then in early 1948 were rather abruptly turned in the opposite direction; and (4) the existence of all kinds of rigidities, either of deliberate or unavoidable character, which made the functioning of the market and price mechanisms extremely inept.

While these abnormalities lasted, it seems obvious that we cannot expect the Japanese economy to exhibit a process of economic fluctuation to which a technique of normal business cycle analysis can profitably be applied.

2. *When Did the Abnormalities Disappear?* When, then, did these abnormalities disappear from the scene? The answer depends somewhat on the nature of the problem we are interested in. If our concern is to account for a remarkable rate of sustained growth, we would need to know when the process of postdefeat rehabilitation can be said to have ended. On the other hand, if we are interested in the character of business cycles in postwar Japan, the fact that the country was, let us say, in the tailend of the rehabilitation process need not necessarily discourage us from applying our tools of business cycle analysis. However, even when the nature of our problem is clearly set, it is not possible, I believe, unequivocally to date the point of time for the end of the rehabilitation process. There can be a number of alternative answers, such as: (a) The year in which per capita real income regained the prewar normal level: 1953 if the average of 1934-36 is taken as "the prewar normal," but 1957 if either 1938 or 1939 is taken as "the prewar normal." (b) The year in which the major sector of the economy regained, in physical output, the level of the presurrender peak. (In 1956 the index of manufacturing production went over the presurrender peak of 1941 and 1944.) (c) The approximate year when the productivity of investment (the reciprocal of over-all marginal capital-output ratio) became normal, having more or less exhausted the possibility of utilizing the pre-existing unused capacities. (As can be seen in Table 1, its smoothed-out magnitude began settling down, in 1953, to what may be considered a more normal value of 60-70 per cent as contrasted to an

erratically high value up to that year.) (*d*) The year in which the real per capita GNP crossed from below Japan's historical trend line. (If we assume 2.5 per cent rate of growth and choose the average of 1934-36 as our base, the estimated achievement of 1960 still falls short of the trend line approximately by 10 per cent.) The hypothesis, to explain Japan's remarkable record of growth in the fifties, will be no doubt affected by the choice we make of the alternative signposts for the end of the rehabilitation process as indicated above.

The second of the abnormalities mentioned above, i.e., the inflation of substantial magnitude, ended, for all practical purposes, by the first quarter of 194°, although the outbreak of the Korean war in June, 1950, gave Japan another inflationary push bigger than the contemporaneous price rises in other countries.

The third of the abnormalities, i.e., the impinging of Occupation policies on the economy, can be said to have lasted, in a general sense, until the Peace Treaty became effective in April, 1952. But since the Occupation authorities themselves, by the end of 1948, started taking the initiative in restoring in Japan the efficacy of price mechanism and the system of free enterprise in general, the abnormality referred to, which may have lingered on beyond 1949, was not of a disruptive character in the latter years. Even the abrupt termination of the GARRIOA aid (Government Aid for Relief and Rehabilitation in Occupied Areas)[4] in June, 1951, did not have any disruptive effect on the Japanese economy since the "special procurement" by the United Nations forces for the Korean campaign smoothed the transition.

The fourth of the abnormalities, i.e., the rigidities born of the extraordinary postdefeat disruption of the economy, is the most difficult from the point of view of dating its disappearance. It is clear that by nature of the case it did not disappear by one stroke. But a most significant turning point was the setting of a single exchange standard in April, 1949, which had an effect of opening the hothouse window, as it were, to let the cold wind of international market forces come into Japan.[5] This step to make the exchange rate effective, co-ordinated simultaneously with the so-called "Dodge disinflation" measures which were carried out with the uncontestable authority of the Occupation administration, gradually brought an order into the market mechanism, rewarding the efficient and punishing the laggard through neutral forces of competition in the market. True, governmental controls of all kinds died

[4] In 1948, for example, the American aid amounted to about 8.2 per cent of Japan's national income, or 179 per cent of her commercial exports.

[5] Until the single exchange standard was established in April, 1949, Japanese exports and imports were transacted externally at world market prices but internally at the prevailing Japanese domestic prices. This meant that there were specific implied ratios of exchange for each commodity, yen-cheap for export goods and yen-dear for import goods, such ratios having a very wide range of 100 yen to a dollar to 900 yen to a dollar.

hard on the one hand and monopolistic arrangements of various sorts came to be revived in due course of time on the other. But the hardship of "disinflation" was soon mitigated by the buoyant windfall of the Korean conflict; and Japan's private business world, it may be said, regained confidence in running its own affairs by the time a recession set in in 1953. Thus it will not be far from the mark if we say that the peculiarly postdefeat abnormalities in the market mechanism of Japan were more or less corrected by the time the recession of 1953-54 ended.

In view of the above discussion on the historical background, one may conclude that for the purpose of growth analysis the significant date to begin it will probably be around 1952-54 or later and that for the purpose of cyclical analysis the coverage of years might be extended a few years further back though certainly not beyond 1949.

Interpretation

1. *The Shift in Structure.* If our judgment is that the process of postdefeat rehabilitation ended for all practical purposes by 1953 or thereabouts, we are still called upon to explain an extremely high rate of growth of 8.2 per cent per annum in real GNP between the three-year average of 1952-54 and that of 1958-60. One possible, and partial, explanation is the sizable shift in the industrial structure of the economy from the low-productivity sectors to the high. One can gauge the possible order of magnitude of the effect of such a shift on the growth rate from a hypothetical example where the relative size of the labor force in the low-productivity sector, having one-half the productivity of the high,[a] shifts from 60 to 40 per cent in six years. Without any change in the total labor force or in the productivity of either sector, real GNP can grow by 2 per cent per annum in such a case. It cannot be denied that the Japanese economy experienced such a shift in recent years; but the maximum that can be accounted for by this factor did not most likely exceed two percentage points per annum. And in any case, such a shift would require the corresponding increase in capital investment.

2. *High Investment and Its Cause.* It is evident that the *sine qua non* of the high rate of growth in the period we are focusing on was the sustained rate of high productive investment; and naturally statistical evidence is not lacking in this regard. The problem is to single out, if possible, the driving force behind it. A detailed analysis of quarterly figures of private fixed investment in major manufacturing sectors reveals that they were correlated much less either with the value of product-sales or with the rate of operation, both in the preceding quarter, than either with the profit after tax or negatively with the rate of interest, again

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[a] Such a disparity may sound somewhat extreme even for illustrative purposes; but it is quite realistic in the context of the present Japanese situation.

both in the preceding quarter.[7] This seems to suggest that private investment in plant and equipment was either autonomous or more a function of the rate of profit than a variable responding to an acceleration mechanism. High marginal efficiency of capital under the condition of price stability appears to have been a central factor in the situation.

If that is the case, we are pushed back further to explain the condition of sustained high marginal efficiency of capital. And I offer here a hypothesis that this was based upon the process of Japan's catching up with her own productivity potential. At the time the single exchange standard was established in April, 1949, it is quite clear that the realized productivity of Japan's manufacturing sector then was far below its own potentiality, owing to the incomplete recovery in external economies and the malallocation of resources consequent to rigidities of all kinds. The period subsequent to 1949 witnessed the recovery in these respects as well as the rapid introduction[8] of the innovations from which Japan had been isolated for about ten years and yet which could be absorbed by Japanese engineers and skilled workers without much delay. In no other way could we explain the trebling of per-man productivity in manufacturing in ten years between 1949 and 1959.

Now, productivity rise is absorbed either by a fall in the price, a rise in the real wage rate, and/or a rise in the rate of profit. What did actually happen? First, the domestic wholesale price level has remained remarkably stable since 1951. Prima facie, this means that the latter two factors shared the fruit of productivity rise. Since all the statistical indicators point to the fact of a distinct lag in the rise of the real wage rate, the favorable effect on profit income appears to be indisputable.[9]

The problem as regards the price level calls for a further scrutiny. Although the average wholesale price level remained more or less stable between 1951 and 1959, industry component indexes are characterized by divergent movements. Whereas domestic prices of textiles appear to have been affected by the international price trend and have moved downward, domestic prices of metals and machinery have shown a rising trend, especially after 1954, and furthermore seem to have been maintained, often independently of export prices, suggesting a widespread practice by the industry of the dual price policy. A direct study on the

[7] See Economic Planning Agency, "Setsubi Toshi to Keiki Hendo" (mimeo., 1959), p. 28. Average correlation coefficients for manufacturing as a whole during the period from 1952 to the second quarter of 1958 are: $+0.769$ with the rate of operation, $+0.287$ with the value of product-sales, $+0.931$ with the profit after tax, and -0.946 with the rate of interest.

[8] The purchase of new foreign patents and the introduction of new foreign technique in general were permitted only in 1950.

[9] The average annual rate of change in the unit labor cost (the index of wage rate divided by the index of productivity) in manufacturing is estimated by the Japanese government to have been $+0.9$ per cent for the U.S.A. between 1953 and 1958 and $+1.2$ per cent for Western Germany for the same period whereas it was -1.8 per cent for Japan between 1953 and 1959. (See *Keizai Hakusho*, 1960, p. 314.)

subject reveals this to have been the case;[10] and it is difficult to escape the conclusion that as regards many exportable commodities the monopolistic price policy was practiced at home while export prices were accommodated to the dictate of international competition. Whether export prices were below cost or not is difficult to tell. But in view of the fact that the 360-yen-to-a-dollar rate, set in April, 1949, was in harmony with the abnormally low productivity situation in Japan at that time, the catching-up process mentioned above must have kept Japanese industries at a competitive advantage under the given exchange rate and under the condition of lagging wage rise. It appears to me more plausible to assume that the apparent stability of domestic wholesale prices under such conditions meant continuous reaping of monopolistic profit by a certain number of industries—the monopolistic profit which became the basis for high plough-back.

The catching-up process, however, has to come to an end some time. If we take the view that the trend line of 2.5 per cent rate of growth in real per capita income starting from the average of 1934-36 indicates roughly the realistic potentiality of the Japanese economy, we can say that, assuming the continuation of a high rate of growth in the immediate future, the catching-up process is likely to be over in a few years' time and that from then on the investment requirement for incremental rise in real output will be more normal.

3. *Sources of High Effective Demand.* High level of productive investment, which in general had an autonomous character in the case of Japan, must have created the supply capacity without necessarily generating sufficient demand. Thus our next problem will be to examine the sources of effective demand which apparently supported the high rate of growth.

First of all, the role of external demand has been of special importance both as regards the growth situation and the cyclical pattern of the development. The cyclical aspect arises not only because of the effect of exports on over-all effective demand but also via the monetary policy which has been geared to preventing the balance of payments from going too far into the red. When industrial activities expand, demand for raw material imports naturally rises; and if exports do not keep pace with imports in such a circumstance, the monetary authority adopts a tight money policy aiming to discourage further expansion. In both 1954 (February) and 1957 (May), the turning point more or less coincided with the effective application of such a monetary policy.

[10] See, in particular, Hitoshi Misonou, "Jiyuka de Kakaku Taikei wa Do Kawaru ka," *Keizai Seminar,* Oct., 1960, pp. 58-61. The ratio of export price to domestic "official standard" wholesale price for steel bars, for example, has recently moved from 80 per cent (Mar., 1957) to 76 (Mar., 1958) to 96 (Mar., 1959) to 89 (July, 1960). The similar ratio for ammonium sulphate at the same dates as above was: 98, 83, 78, and 78 per cent, respectively.

In terms of the growth situation, the Korean conflict and the subsequent maintenance of "special procurement" demand were a distinct boon. (See Table 1 for the proportion of "special procurement" to the gross total receipts from abroad on goods and services.) But probably more important was the favorable cost situation of Japanese manufacturing industries due to the catching-up process discussed above. So long as the real wage rate does not quite catch up with the recovered potentiality of Japan's manufacturing industries, her exports will enjoy this cost advantage under the given exchange rate fixed in 1949.

Second, a number of important shifts can be indicated as regards consumers' behavior in the postwar Japan. In particular, the land reform, a part of the Occupation democratization program, contributed to transforming the rural life from the one largely characterized by poverty-stricken tenants to the one dominantly of small landowners who came to constitute a significant part of domestic market. This transformation probably had a boosting effect on the over-all demand situation more in the period of 1949-52 than in the latest. Then the tremendous upsurge in the propensity to purchase consumer durables, coupled with the coincidental introduction of consumers' credit in a number of new fields, is undoubtedly of major importance, especially in the last several years. It is to be noted that the fashion of consumer durables is having a doubled impact in Japan inasmuch as those of prewar vintage (refrigerators, vacuum cleaners, automobiles, etc.) and those of postwar (television, room-coolers, etc.) are converging on Japanese households at the same time. Trend of home builders to turn more and more towards non-flammable, more durable, and thus more expensive, types of houses, making use of the products of high-productivity heavy industries, has also the effect of increasing the construction expenditure per unit of residential need, as well as creating a large new demand for heavy industry products.

Third, investment expenditures, while having the capacity increasing effect on the one hand, are no less important as a source of effective demand. While the fixed investment up till about 1954 was characterized mainly by the rehabilitation investment and by scattered cases of modernization, the investment in plant and equipment since 1955 has had a distinct slant in the direction of innovational investment. Thus if we choose as peculiarly innovational the following six fields, electrical and electronic equipments, automobiles, plastics, petro-chemicals, synthetic fibres and the atomic power, and if we calculate the ratio of fixed investment in them to the total such investment in manufacturing as a whole, the ratio of 15.4 per cent in 1954 and 14.9 per cent in 1955 registers a rise, annually from 1956 to 1959, to

22.5, 27.8, 31.8, and 34.9 per cent.[11] Innovational investment in such fields cannot be fruitful unless more basic heavy-industry sectors of the economy are capable of supplying intermediate goods of needed high quality. This process of modernization, in such sectors as steel, chemicals, and heavy engineering industries, preceded the flowering of innovational investment and became accelerated, since 1956, as they found a greater and greater demand at home for their improved products. Thus "the complementarity effect of investment" (A. Hirschman) is clearly indicated here; and the skyrocketing increase in private investment on plant and equipment in 1956 and 1957 and also in 1959 can be concretely traced to such an effect.

Finally, we may take a look at that major offset to deflationary gap: defense expenditures in peacetime. If they are substantial, they can help sustain the effective demand, but at the same time they will divert resources from otherwise feasible productive investment to the consumptive type of expenditures. In Japan's case in the fifties, the expenditure on defense, which was started on her own in 1950, was large enough, one might say, to assure a continuing stable market for heavy industries which were just then on their way to recovery but was not large enough to drain resources away from productive investment. The ratio of budget expenditures on defense to GNP was 0.5 per cent in 1950, rose to 2.0 per cent in 1952, and thereafter followed a declining trend to 1.1 per cent in 1959.

4. *Other Considerations.* In addition to the above considerations in explanation of Japan's high rate of growth in recent years, one should probably mention, also, two other factors of some importance. First, the flexibility of supply of labor force has thus far been quite satisfactory. In spite of the extremely low ratio of unemployment to labor force throughout the postwar period (the range of 0.7 to 1.5 per cent between 1947 and 1959), the volume of disguised unemployment is known to have been quite high, and in fact there has not been any difficulty for industries until quite recently to find additional labor force, even of qualified type, needed for expansion. The fact that Japanese workers started, in the postdefeat period, from an extremely low real wage level must have helped a great deal in bringing about a labor supply function favorable to industries' needs. Even if the present high growth rate continues, the pressure of demand on labor market is not likely to be felt until 1967 or thereafter, inasmuch as the postwar concentration of child-births is expected to yield an exceptionally high net increase in labor force until 1966. Furthermore, if the labor market is really pressed, the peculiarly Japanese insti-

[11] Based on a study by the Ministry of International Trade and Industry. The figure for 1959 is preliminary.

tutional barriers against labor mobility will no doubt weaken, and a situation favorable to more rational allocation of labor resources is likely to ensue.

Second, the role of the government cannot be abstracted from Japan's growth process in the fifties. Although the so-called "Dodge Line" of 1949 did introduce into the Japanese scene important elements of competitive framework (such as the single exchange standard, the orthodox monetary policy, the negative attitude on government investment in industries, the use of budget surplus for debt retirement, the strengthening of antimonopoly measures, etc.), it was on the whole shortlived except as regards a few measures that could not easily be reversed. Especially after April, 1952, when Japan regained her independence, it will be difficult to regard Japan, as Mr. Egon Sohmen does,[12] as an example of an economy able to achieve a high rate of growth because of its competitive structure. Monopolistic practices came back early; and the government embarked on a gigantic industry-financing program through a number of governmental development corporations, as well as introducing numerous tax-exemption or tax-relief measures aimed at specific industries and investment programs. The legacy of wartime high tax rates, which take time for eventual relaxation, has helped the government to enjoy relatively ample financial resources; and the level of treasury investments and loans has lately been of the magnitude fully comparable to the total retained income of corporations. The government has made use of such resources quite effectively now for one industry and now for another, shifting the emphasis as the circumstances demanded. The extent of corporate tax relief, on the other hand, is indicated by the rising ratio of the tax-exempt portion of corporate gross profit to the retained income of corporations. While the denominator itself expanded, the ratio rose from 9 per cent in 1951 to 113 per cent in 1952, 152 in 1953, and 213 in 1954.[13] It appears to be certain that were it not for such a sharing of risk by the government several of the essential industries would not have achieved the level of investment that was recorded in the fifties.

Future Outlook

As we review Japan's process of growth in this manner, there is not much mystery in the exceptionally high rate that has been attained between 1952-54 and 1958-60. There remains, however, a question of the future. If my preceding analysis is correct, one important element

[12] See Egon Sohmen, "Competition and Growth: West Germany," *A. E. R.*, Dec., 1959, especially p. 1001.
[13] See Economic Planning Agency, *Sengo Nihon no Shihon Chikuseki to Kigyo Keiei*, 1957, p. 156.

in the situation, i.e., what I called the "catching-up process," is soon to become a negligible factor. Even then, major shifts in production functions are yet to come in a number of industries, as well as the further shift upward in the demand for consumer durables. The prospect, therefore, is still the feasibility of continuing a fairly high rate of growth in the coming decade, with, however, the following qualifications: (1) that it is likely that a high growth rate will depend more and more on the demand situation than on the realized profit rate from now on; (2) that, in particular, the doubled efforts for the maintenance and expansion of Japan's export markets will be required; (3) that the social overhead structures, providing external economies for the private sector, will demand a greater share of the total capital needs in the future[14]; and (4) that it will become increasingly important to break the peculiarly Japanese institutional barriers against labor mobility.

Whether a high growth rate can be maintained with reasonable stability of the economy or whether a high growth rate, as such, will be able to solve various structural problems Japan suffers from[15] is, I believe, a different problem.

[14] The proportion of social overhead capital in the total reproducible physical wealth of Japan has been declining steadily since 1946; i.e., from 51.0 per cent in 1946 to 42.2 per cent in 1957. (See Economic Planning Agency, *Sengo Nihon no Keizai Seicho*, 1959, p. 12.)

[15] In particular, the problem of so-called "dual structure"; i.e., the coexistence of and the lack of labor mobility between the modern high-wage sector with large size on the one hand and the technically-lagging low-wage sector with small size on the other.

DISCUSSION

GARDNER ACKLEY: These are three extremely interesting and valuable papers. Particularly since (in contrast to the usual situation) these papers were all available to the discussants considerably in advance of this meeting, a discussant might well be expected to use this opportunity to suggest some generalizations covering the postwar business cycle in the free-world economies. Or, at least, he might point up some of the more striking contrasts among the experiences of these three areas or between the experiences of these areas and the postwar experience of the United States. For example, these papers insistently raise the question why the United States has done so poorly in achieving stable and rapid growth. Is it a matter of economic structure, or, as one of our authors suggests, also partly a fault of our economic policy?

On a narrower scale, a discussant might propose some comparisons between the experiences of the Italian and Japanese economies, so alike in many basic respects: for example, in population and natural resources, in their incomplete modernization, and in their dependence on international trade. Both were defeated countries which first made spectacular recoveries, but, without pausing when recovery was complete, have continued with equally spectacular growth.

Unfortunately, my time and talent have not been sufficient to permit me to propose any striking or penetrating generalizations or contrasts beyond those obvious to each reader. And, besides, my comparative advantage relates primarily to one of these economies. Thus, with apologies to the authors and the readers, I propose to direct my few remarks primarily toward Professor Hildebrand's paper on the Italian experience.

Professor Hildebrand provides us an excellent summary both of the Italian postwar economic achievement and of Italy's still pressing problems of unemployment and incomplete development. But there are aspects of his analysis that I propose to question. These involve propositions that are rather widely accepted by students of the Italian economy, both Italian and foreign. They are propositions which I have myself accepted and expounded in the past; yet which seem to me more doubtful on further study.

Briefly, it is widely agreed that the grievous unemployment of Italian labor is not a product of deficient aggregate demand but stems primarily from a shortage of physical capital. It is not, therefore, remediable by measures directed toward aggregate demand but rather can only gradually be overcome as capital is accumulated. Although I have some reservations about this formulation, it is not this proposition toward which I direct my comments, but the next several. "Very rapidly rising" industrial wage rates (says Hildebrand) have reinforced an already "perverse" tendency for Italian investment to be highly capital-intensive. Although such investment has permitted an extremely rapid increase in industrial output, it has provided very little increase in employment in the modern sectors. Thus any employment increase has been forced to occur in the more primitive sectors, intensifying the income differential, perpetuating the dualism of the economy, and permitting only a painfully slow approach toward full employment.

Let me quickly make a few points regarding this argument.

I do not believe that there is any evidence that the wage-rate increase in the industrial sectors has been excessive or dangerous. Hildebrand himself recognizes that the productivity increase has been substantially greater than the wage-rate increase, thus permitting stable or falling industrial prices and, at the same time, very attractive profits. While it might have been better if industrial prices fell somewhat more and wages rose somewhat less, the inevitable imperfection of competition in the modern heavy sectors makes this a largely futile hope. So long as industrial prices have been stable or falling and profits have been adequate or more than adequate, I believe that the wage increases have performed a useful role in expanding mass markets. Nor do I believe that they have provided much inducement to any undesirable capital-intensification.

It is not even clear that there has been any capital-intensification in the sense to which Hildebrand refers. Data on marginal capital-to-output ratios are now reasonably available. If we take the ratio of cumulated real gross investment in fixed assets over the six years 1952-57 to the six-year change in gross output from 1952 to 1958, we get an over-all ratio of about 4 to 1. Rather high. But let us look at the composition both of the investment and of the output increase. In this period, 25 per cent of the investment was in dwellings, which, because of the state of existing housing, produced little net gain in housing services. In fact, if we believe our figures, the marginal gross capital-to-output ratio in housing was 160 to 1! Another 10 per cent of investment was in public works, whose pay-off is remote and not necessarily measurable. An additional 22 per cent of investment was in public utilities and transportation, with a marginal capital-to-output ratio of almost 10 to 1.

Now the foregoing investment, 57 per cent of the total, is all highly capital-intensive. Had it not been made, there might have been larger investment in the sectors that provide more jobs. But one can hardly lay the blame for the capital-intensive investment in housing, public works, and public utilities to high industrial wage rates. And the public works and public utility investments have a crucial role in general development and in the effort to stir the South from its centuries-old lethargy.

Twenty-seven per cent of total investment was in the manufacturing, construction, and mining industries. This sector contributed 57 per cent of the total output increase, with a marginal capital-to-output ratio of only 1.9 to 1, not only substantially lower than the ratios in housing and public utilities, but also lower than the ratios in the remaining sectors: agriculture (3.3); trade and miscellaneous services (2.0). Agriculture received 13 per cent of total investment. While relatively capital-intensive, this investment was and is unquestionably necessary for modernization.

It has been commonly accepted that there has been little or no growth in employment in the modern sectors. But, as Hildebrand admits, employment data have been meager and of poor quality. A year ago, the first complete and consistent set of employment estimates were presented in a monograph prepared by SVIMEZ, a highly respected research organization. These estimates show that between 1950 and 1957 total employment expanded by 8.2 per cent. Manufacturing employment, however, by these estimates, grew by 12.5 per

cent, at a faster rate than for the total economy. While agricultural employment decreased by 8.4 per cent, nonagricultural employment grew by 19.6 per cent, with the largest percentage increases coming in transportation and communications, construction, and in machinery manufacturing. Of the total of almost 2 million new nonagricultural jobs, 64 per cent were in the largely modern sectors: 26 per cent in manufacturing, 21 per cent in construction, and 17 per cent in public utilities, including transportation. I wonder to what extent the generalization about high wage rates leading to investment which expands output but not employment may rest merely upon meager and misleading estimates of employment. The SVIMEZ estimates stop in 1957. Since then, manufacturing employment has surely gained considerably, as evidenced by sample surveys and, dramatically, by the massive recent migration into the northern industrial cities.

On the basis of the evidence I am able to put together, I see nothing perverse or unhealthy in Italian development. It is still incomplete; a dual economy still exists. But there is no convincing evidence that the income differential between the more primitive and the more modern sectors has been widening. However, the modern sectors are expanding rapidly both in output and in employment, and at a level of efficiency which permits their easy incorporation into the Common Market economy. Such inflation as has occurred can largely be laid to import prices and to the increasing gap between wholesale and retail prices, as a consequence of the archaic Italian distribution system, which cries out for reform and progress. In no sense do I see signs of a serious wage-push on the price level. Certainly one can hardly approve, as both Hildebrand and I do approve, of the relatively loose rein of monetary and fiscal policy and at the same time complain about wage-rate increases which have fallen short of the productivity increases in the modern sectors.

The spectacular postwar growth rates of the Italian and Japanese economies show how fast a partially developed country can grow, given reasonably intelligent monetary and fiscal policies. This almost explosive growth should offer real hope to the economies struggling to get development started.

The United States economy, already fully developed, surely cannot be expected to grow at the rates shown by postwar Japan and Italy. But this is no reason for us to be satisfied or complacent about our own slow and halting growth. It is clear that it is not only in the Communist world that our performance has been surpassed.

L. M. Koyck: The postwar experience of the performances of West European economies and also of the Italian and Japanese economies is considerably different from its prewar experience and also different from the postwar experience of the U.S. economy. Two main points of difference are as follows: (1) the relatively high rate of growth of Western European, Italian, and Japanese economies; and (2) the relatively mild and short depressions, sometimes only characterized by a slower rate of growth instead of a downturn of national output.

To comment on these points, which are not independent but closely related, it is useful to apply the analytical framework of Dr. Lundberg's paper and to

distinguish the effects of (*a*) the external or autonomous impulses on the national economies, (*b*) the structural characteristics of the economies and, especially, (*c*) the governmental policies applied.

Growth, Stability and the Role of Autonomous Factors. The high amount of more or less autonomous investments in the countries with considerable war damage, investments related with technological progress, the catching-up with the pool of cost-reducing techniques and new products accumulated during and after the war seem to be important factors in the explanation of the high rate of growth. The high investment ratios in Italy, Japan, and West European countries are consistent with this assumption. The papers of Dr. Hildebrand and Dr. Shigeto Tsuru gave me the impression that something of a catching-up process has been important in the Italian and Japanese economies. If this is correct, this may give rise to the expectation that it will be unlikely that the same high rate of growth will be maintained for the next decade or so.

Investments related with the introduction of new products or with cost-reducing techniques of production are relatively insensitive to fluctuations in demand. A high proportion of this type of investments in an economy may reduce the sensitivity of the economy to external impulses. The volume of exports—one of the most important external impulses in small countries—fluctuated less than in the period between the wars in the Scandinavian countries, as mentioned by Dr. Lundberg.

Stability and the Role of Government. There is little doubt that governmental tax and social security legislation has introduced a certain amount of automatic stabilization in many countries. This legislation has reduced the multiplier effect of external disturbances. Especially, the impact of any change in aggregate income on total consumers' expenditure has been reduced. Some calculations for the U.S. economy, as far as I remember, showed a decrease in the multiplier of exogenous expenditure from nearly two in the thirties to about 1.5 in the fifties. In many other countries, something of this type of structural change will also be relevant.

There are also structural changes with opposite, i.e., destabilizing, effects, which should be mentioned. The introduction and rapid growth of consumer's installment credit is important in this respect. The consequences are: First, some increase in the marginal propensity to consume seems likely. This implies more violent reductions of consumption following a fall in incomes, or generally, a higher cyclical sensitivity of consumers' expenditure. Second, it increases the possibility of random shifts in consumers' expenditure, the "budget restraint" becoming less stringent for consumers. Though these changes in consumption at given levels of income, debt, and liquidity may be relatively small, the absolute amount of the shift may be a considerable impulse to the economy.

Dr. Lundberg drew our attention to some destabilizing effects of governmental action. Apart from the well-known destabilizing effects of a wrong timing of anticyclical governmental policies, there are some other experiences, which, I think, may be important. These refer to public investments or publicly-controlled investments as an instrument of anticyclical policy. To counteract cycles in the field of private investments, this policy should introduce cycles,

of opposite direction, in public investments. A good example is the Norwegian building cycle, mentioned by Dr. Lundberg. Though, if rightly timed, this contributes to the stabilization of the whole economy, it is not without costs to introduce intsability in the field of public investments. The "policy-induced" fluctuations in house building and public investments can entail considerable losses: the unemployment of equipment and even labor.

There is another experience worth mentioning. This has to do with the structural disequilibrium which, under certain conditions, may develop as a consequence of a stabilizing public investment policy. When there is a continuous threat of excess demand, as, for example, was the case in the Netherlands in the after-war period, there is a constant pressure on public investments by governmental stability policy. Public investments (excluding housing) fall from an average level of about 25 per cent of total investment in fixed capital before the war to about 15 per cent in the fifties. The idea of catching up with the public investments during depression periods did not work very well because there were no major depressions.

Problems of structural disequilibrium may arise in this situation when public and private investments are complementary. This may well be the case, for example, in the transportation sector of the economy. In the Netherlands, as in many other countries, there was a very rapid increase in private investment in transportation equipment in the fifties. Complementary to these investments are public investments in highway construction, the building of bridges, etc. When public and private investments are more or less complementary, a restriction of the first type when the second type is rapidly increasing implies a less efficient allocation of resources from a welfare point of view. There "costs" should be considered in the choice between alternative stabilization policies.

THE BALANCE OF PAYMENTS OF THE UNITED STATES: PROBLEMS AND PROSPECTS

DISTURBANCES AND ADJUSTMENTS IN RECENT U. S. BALANCE-OF-PAYMENTS EXPERIENCE

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Large deficits have been registered in the U.S. balance of international payments during the past three years despite the fact that, in relation to levels obtaining earlier in the fifties, economic activity in most other industrial countries has been much higher than in the United States. This circumstance has contributed to a widespread view that the play of demand forces associated with these levels of economic activity has been counteracted by unfavorable developments in relative prices at home and abroad and other changes causing a deterioration in this country's competitive position in world trade.

This paper embodies the view that such general forces do not suffice to explain recent U.S. balance-of-payments experience and calls attention to a number of specific disturbances which, coinciding in a relatively brief period, have constituted a major adjustment problem. In consideration of these disturbances, the basic trade and payments position of the United States appears stronger and its capacity to adjust greater than the size of the recorded deficits would suggest.

I. *Temporary Fluctuations and Short-term Capital Flows*

In abstract models of the balance of payments the disturbance factor is given by assumption. Thus, starting from equilibrium, a country begins lending abroad, and the theorist then traces subsequent repercussions and adjustments. Actual experience sometimes seems to approximate the convenient simplicity of the classical model. But a more complex picture emerges when one examines in some detail the course of U.S. trade and payments over the last several years, as reflected in Tables 1, 2, and 3. There is no clear position of equilibrium to start from, everything seems to be in motion, and no one element stands out as dominant over the others.

Upon closer inspection, some of this apparent confusion is attributable to cyclical fluctuations, which have not followed parallel courses at home and abroad. One sees also the pronounced effects of other

TABLE 1

U.S. BALANCE OF PAYMENTS, 1953–55 TO 1960*

(BILLIONS OF DOLLARS)

	1953–55 (Annual Average)	1956	1957	1958	1959	1960, by Quarters Seasonally Adjusted Annual Rates		
						I	II	III†
U.S. payments	$21.0	$26.3	$28.0	$27.8	$29.0‡	$29.7	$30.7	$31.5
Merchandise imports	11.0	12.8	13.3	13.0	15.3	15.2	15.4	14.9
Services (excluding military expenditures)	3.2	4.1	4.5	4.7	5.2	5.5½	5.7½	5.6½
Military expenditures	2.7	3.0	3.2	3.4	3.1	3.0½	3.0½	3.1½
U.S. grants and capital	4.1	6.5	7.1	6.7	6.1	6.1	6.6	8.0
Remittances and pensions	.6	.7	.7	.7	.8	.8	.9	.9
Government grants	1.8	1.7	1.6	1.6	1.6)			
Government loans and credits	.6	1.1	1.6	1.5	1.4)	5.3	5.7	7.1
U.S. private capital, net	1.1	3.0	3.2	2.8	2.3)			
Of which, short-term, net	.2	.5	.3	.3	.1	.2½	.6½	1.8½
U.S. receipts	19.1	24.7	27.7	23.9	25.0	27.3	28.4	28.5
Merchandise exports	13.1	17.4	19.4	16.3	16.2	18.3	19.5	20.0
Services (excluding military transactions)	5.0	6.2	7.0	6.8	6.9	7.2½	7.2½	7.4½
Military transactions	.2	.2	.4	.3	.3	.3½	.3½	.4½
Repayments on U.S. government loans	.5	.5	.7	.5	1.0	.7	.6	.7
Foreign long-term investment in U.S.	.3	.3	.4	.1	.5	.7	.6	.0
Excess of receipts (+) or payments (−) on all recorded transactions listed above	−1.9	−1.6	− .2	−3.9	−4.6	−2.4	−2.2	−3.0
Goods and services (excluding military)	+3.9	+6.7	+8.6	+5.4	+2.7	+4.9	+5.6	+7.0
Military services	−2.5	−2.8	−2.8	−3.1	−2.8	−2.7	−2.5	−2.7
Grants and capital	−3.3	−5.5	−6.0	−6.1	−4.5	−4.6	−5.3	−7.3
Errors and omissions	.3	.6	.7	.4	.8	− .1	− .6	−1.1
Increase in foreign gold and liquid dollar assets through transactions with the U.S.	1.6	1.0	− .4	3.5	3.8	2.6	2.8	4.1

* Transfers of military aid are excluded both from exports (under receipts) and from grants (under payments).
† Preliminary.
‡ Data for 1959 exclude the special payment of 1,375 million dollars on the U.S. subscription to IMF.
§ Seasonal adjustments of these items are not separately shown by the Department of Commerce and have been made by the author.
NOTE: Detail may not necessarily add to totals because of rounding.
SOURCE: Department of Commerce.

temporary influences such as the Suez crisis of 1956-57 and its aftermath, the protracted U.S. steel strike of 1959, shifts in U.S. cotton export pricing policy, and still others. In light of the renewed strength of U.S. exports in 1960, it can now be seen that the size of the deficits registered in 1958 and 1959 was due in considerable part to an exceptional concentration of adverse factors of a temporary nature.

More recently, the United States has experienced a heavy outflow of short-term capital. The movement has been largely in response to the rise in interest rates abroad and the fall in this country during the past year, though this flow and the attendant losses of gold have no doubt sparked other transfers of a speculative nature. These short-term capital movements again plunged the balance of payments heavily into deficit in the second half of 1960—at least on the method of accounting employed by the Department of Commerce.

On this accounting aspect of the question, it may be observed that there is a considerable range of choice in any decision as to which

TABLE 2

U.S. EXPORTS DISTRIBUTED ACCORDING TO PERCENTAGE INCREASE IN VALUE,
1953–55 TO 1960

(MILLIONS OF DOLLARS)

	1953–55 (Annual Average)	1957	1958	1959	1960 April–September* (Annual Rate)	Percentage Increase, 1953–55 to April–September 1960
Exports, excluding "special category," total....	*$12,995*	*$19,299*	*$16,134*	*$16,156*	*$19,394*	*49%*
Class I—*Increases greater than two-thirds:*						
Total, Class I...........................	1,516	2,630	2,143	2,243	3,824	152
(Per cent of total exports).............	(11.7%)	(13.6%)	(13.3%)	(13.9%)	(19.7%)	
Aircraft and engines.....................	114	269	217	160	716	528
Iron ore and scrap.......................	111	375	130	200	350	215
Nonferrous metals and materials.........	268	448	348	299	792	196
Chemicals and related products..........	723	1,092	1,060	1,184	1,424	97
Military equipment† and unclassified items	300	446	385	400	542	81
Class II—*Increases between one-third and two-thirds:*						
Total, Class II..........................	7,580	11,429	9,859	9,945	11,434	51
(Per cent of total exports).............	(58.3%)	(59.2%)	(61.1%)	(61.5%)	(59.0%)	
Food....................................	1,855	2,746	2,555	2,795	3,066	65
Capital equipment, excluding transportation items.............................	2,610	3,956	3,580	3,622	4,082	56
Raw cotton..............................	595	1,058	662	452	885‡	50‡
Industrial materials not separately listed..	1,543	2,113	1,955	2,108	2,246	46
Miscellaneous consumer manufactures.....	448	563	544	596	616	38
Iron and steel-mill products.............	529	993	563	372	722	36
Class III—*Increases smaller than one-third:*						
Total, Class III.........................	3,901	5,240	4,132	3,968	4,136	6
(Per cent of total exports).............	(30.0%)	(27.2%)	(25.6%)	(24.6%)	(21.3%)	
Consumer appliances.....................	244	276	302	303	320	31
Automotive parts and accessories, tires, and tubes.............................	495	635	587	640	642	30
Tractors, trucks, and buses.............	686	901	663	735	814	23
Drugs and medicinals....................	229	284	278	285	278	21
Railroad equipment......................	110	145	208	103	132	20
Textile materials and manufactures excluding raw cotton........................	633	684	620	657	698	10
Inedible fats and oils and oilseeds........	127	167	117	150	138	9
Coal and related products...............	384	846	534	388	412	7
Petroleum and products..................	667	994	557	480	508	−24
Passenger cars..........................	326	308	266	227	164	−50

* Individual items, classes, and groups for April–September, 1960, are not seasonally adjusted (see, however, footnote ‡ regarding cotton). The seasonally adjusted total for this period is 19,520 million dollars (annual rate).
† Government cash sales.
‡ The figure for cotton is the expected total for 1960, according to estimates obtained from the Department of Agriculture. Actual exports in the April–September period, the seasonal low, were 702 million dollars (annual rate), and this figure is used in the total for Class II and in the grand total (unadjusted) for the period.
SOURCE: National Bureau of Economic Research, based on data supplied by U.S. Department of Commerce.

classes of transactions are to be entered in the balance of payments proper and which are to be treated as offsets and hence as a direct way of measuring the surplus or deficit. National practices do, in fact, differ widely and sometimes with mutually inconsistent results. The Commerce Department's treatment may be described as asymmetrical in that all foreign short-term and other liquid assets here, along with gold transfers, are regarded as a measure of the U.S. deficit, or surplus, whereas changes in American short-term assets abroad are, like long-term capital, entered in the balance of payments proper. This treatment has the merit of showing changes in U.S. gold reserves and in potential foreign claims on them but, in

TABLE 3

U.S. Imports Distributed According to Percentage Increase in Value, 1953–55 to 1960

(Millions of Dollars)

	1953–55 (Annual Average)	1957	1958	1959	1960 April–September* (Annual Rate)	Percentage Increase, 1953–55 to April–September 1960
General imports, total	$10,824	$12,952	$12,867	$15,212	$14,746	36%
Class I—Increases greater than two-thirds:						
Total, Class I	2,794	4,716	5,284	6,598	6,560	135
(Per cent of total imports)	(25.8%)	(36.3%)	(41.1%)	(43.3%)	(44.3%)	
Passenger cars, parts and accessories	66	337	547	844	572	767
Iron ore	131	286	233	313	404	208
Iron and steel-mill products	151	220	231	573	430	185
Textile consumer goods—apparel, rugs, etc.	160	250	251	355	450	181
Capital equipment	234	412	481	614	612	162
Noncommercial and unclassified items	165	290	377	410	422	156
Miscellaneous consumer manufactures	673	937	903	1,226	1,464	118
Meat and cattle	189	250	465	476	412	118
Finished textile fabrics and materials	150	200	184	249	288	92
Petroleum and products	875	1,534	1,612	1,532	1,506	72
Class II—Increases between one-third and two-thirds:						
Total, Class II	577	677	738	771	846	47
(Per cent of total imports)	(5.3%)	(5.2%)	(5.9%)	(5.1%)	(5.7%)	
Alcoholic beverages	161	217	237	270	264	64
Sugar	416	460	521	501	582	40
Class III—Increases smaller than one-third:						
Total, Class III	7,452	7,589	6,825	7,843	7,340	− 2
(Per cent of total imports)	(68.8%)	(58.5%)	(53.0%)	(51.6%)	(49.8%)	
Chemicals and related products	230	242	245	290	288	25
Food not separately listed	898	873	938	1,023	1,052	17
Industrial materials not separately listed	2,416	2,543	2,199	2,931	2,698	17
Newsprint and paper base stocks	948	1,032	989	1,087	1,104	16
Materials for farm use, excluding chemical fertilizers	281	282	269	261	246	−12
Nonferrous ores and metals	1,242	1,242	992	1,150	956	−23
Coffee	1,437	1,375	1,173	1,092	996	−31

* Individual items, classes, and groups for April–September, 1960, are not seasonally adjusted. The seasonally adjusted total for this period is 14,950 million dollars (annual rate).

Source: National Bureau of Economic Research, based on data supplied by U.S. Department of Commerce.

times of large outflows of U.S. short-term funds as at present, is not properly indicative of the more fundamental developments in this country's international position. Undue stress on the reported deficit as such, without full regard for all relevant circumstances, can therefore itself engender increased speculative activity and even lead to confusion on questions of policy.

Particularly in view of its self-aggravating properties, the outflow of short-term capital looms as the most immediately critical issue and yet one which, in principle, would appear most amenable to policy. The problem is to adjust policies so as to reduce incentives to short-term capital outflows without at the same time depriving the domestic economy of the stimulus needed in its present lethargy. At least a partial answer could be provided if it were possible to increase interest rates particularly relevant to capital outflows, chiefly the Treasury bill rate, and to reduce those rates which matter most to domestic business, especially mortgage rates and commercial bank lending

rates. It seems likely, however, that less reliance will have to be placed on easy money in general and more on fiscal and other means of stimulating the domestic economy as long as the external accounts remain under pressure.

II. *Disturbances of a More Basic Nature*

It is also necessary that the appraisal of this country's basic international position should not be obscured by the current outflow of short-term capital and the various temporary fluctuations of recent years previously mentioned. The present analysis accordingly focuses on changes in trade and long-term capital movements over a somewhat longer interval. For this purpose, 1953-55 is taken as a base—a relatively quiet period in international trade falling between the end of the disturbances associated with the Korean conflict and those released by the Suez crisis. April-September, 1960, will be taken as a terminal period, comprising the latest two quarters for which balance-of-payments results are available.

As frequently mentioned, the U.S. balance of payments was already in deficit in 1953-55 to the extent of some 1½ billion dollars annually on the average. An excess of payments was, however, a necessary means of redistributing part of the swollen gold stocks which the United States acquired during and after the war and could scarcely be regarded as evidence of disequilibrium any more than, say, exports of newly mined gold by South Africa. A much more relevant point, with favorable rather than unfavorable connotations for this country's competitive performance, is that the unbalanced structure of payments necessary to accomplish this redistribution of reserves became part of the subsequent adjustment problem which the United States has had to face because of new disturbances in its international accounts.

Transactions in Goods and Services. Identification of these new disturbances must take account of the fact that, during the six years from midpoint to midpoint of the base and terminal periods, total payments in both directions (exclusive of transfers of gold and short-term capital) have risen by close to 50 per cent. The analysis must therefore be conducted in terms of relative rates of change as contrasted with the static equilibrium usually assumed in classical models.

Accordingly, in Tables 2 and 3 merchandise exports and imports are broken down into some twenty relatively homogeneous groups arrayed according to rates of increase in value from 1953-55 to April-September, 1960. The difference between the distribution of exports and that of imports is striking. Items comprising 70 per cent of U.S. exports (Classes I and II) in 1953-55 have all shown increases

of more than one-third and, all together, an increase of two-thirds in value. Conversely, items comprising almost 70 per cent of U.S. imports (Class III) in 1953-55 have all shown increases of less than one-third in value and, all together, an increase of only 6 per cent even if coffee is left out because of the sharp fall in its price over the period. What this means is that the good or excellent performance of the greater part of U.S. exports has been accompanied by very poor results for a much smaller group (Class III), whereas the mediocre performance of the greater part of U.S. imports has been accompanied by outstanding results in certain items previously of limited importance (Class I).

This pattern of development does not necessarily disprove the idea that inflation in some sense is the root of the trouble. But it does seem more compatible with the view that the major adverse changes experienced in trade have resulted from structural shifts strongly affecting supply and demand schedules for particular products.

The strengthened position of foreign manufacturers in the U.S. market is evidenced by the growth in imports of finished goods over the last six years: capital equipment up by 160 per cent, wearing apparel by 180 per cent, miscellaneous consumer manufactures by 120 per cent, and automobiles by more than 700 per cent. A stimulating new study by MacDougall [1, pages 41-44] suggests that this growth may have been more rapid in the last few years than can be expected over the longer run because foreign manufacturers have been catching up after being effectively excluded from the U.S. market for a quarter of a century by the abnormal circumstances of the thirties and those of the war and early postwar years.

These changes, however, have not all been in one direction. The rise in imports of capital equipment has been striking but dwarfed in value by the rise in machinery exports. In chemicals, the increase in imports is moderate, in both relative and absolute terms, compared with the impressive gains in exports. Both of these major groups appear to reflect growing international specialization but show little evidence of any deterioration in U.S. competitive capacity, whether due to inflation, the "closing of the technological gap," or anything else. Nor, to take another test, has the U.S. share declined in total world exports of machinery and chemicals.

Still within the area of producers' goods, the position of steel may be rather different. Imports, especially of low-value items like reinforcing rods, seem to have made deep inroads, while the rise in U.S. steel exports since 1953-55 is no more than can be accounted for by price increases and has been largely confined to specialized types, such as sheet metal for automobiles, in which European capacity is

inadequate. Here again one can perhaps see a trend toward greater international specialization, but the shift may well have been accelerated by the sharp rise in American steel prices.

In contrast to producers' goods, the development of trade in consumer manufactures has been much more one-sided in favor of imports. In some items, the growth of imports reflects the increased sophistication of the American consumer. Probably to a greater extent it reflects lower production costs abroad. It should not be too quickly concluded, however, that these cost differences necessarily result from greater inflation here than elsewhere. As far as factor costs are concerned, most of the wearing apparel and miscellaneous consumer manufactures now being imported in quantity could have successfully competed in the U.S. market at any time in the past. The significant change probably lies rather in the new accessibility of this market and in the organization of production and sales to cater to it.

The drastic deterioration of the U.S. trade balance in automobiles may also evidence both new consumer tastes and differences in production costs. Perhaps more important is simply the availability from abroad of a smaller car with lower operating costs and slower obsolescence than American manufacturers were, until recently, providing.

Market forces alone, however, do not explain the lag in U.S. exports compared with imports of consumer manufactures. Whereas this country's commercial policy regarding these imports has been far more liberal than in the days of the Hawley-Smoot tariff, its exports of consumer manufactures could not, at least until very recently, have been sold to most major foreign markets at any price. A considerable relaxation of overt discrimination has now been obtained, but many hindrances and rigidities still remain.

Some very rough estimates of the adverse impact of these various factors on the development of exports and imports may now be ventured, allowing only for marked deviations from the over-all rate of increase in U.S. foreign transactions. The impact would probably be in the order of 1 billion dollars in automobiles alone, including the failure of U.S. exports to share in the growth of the world market, and perhaps only little less than this in other consumer manufactures. Among producers' goods, only steel comes into question, and here the impact appears to be something like 300 million dollars. In addition to the items already discussed, the balance of trade in fuels has been strongly affected by structural shifts or, more specifically, by changes in market organization, underlying the swift rise in imports of petroleum up to 1958 and the new barriers to U.S. coal exports to Europe.

Finally, some allowance would need to be made for exceptionally rapid increases in U.S. payments for various services (exclusive of military expenditures abroad). All told, the adverse impact of these different factors could scarcely be put at less than 3 billion dollars, roughly one-fourth of it on the side of exports and three-fourths on the side of imports of goods and services.

Long-term Capital and Other Nontrade Items. The increases of some 15 per cent in U.S. military expenditures abroad and of 30 per cent in U.S. government loans, credits, and grants, since 1953-55 are too moderate, in relation to the total rise in our international turnover, to be considered as fresh sources of disturbance within the period considered.[1] U.S. private long-term investment abroad, on the other hand, has risen much more sharply from 850 million dollars in 1953-55 to over 2 billion in each succeeding year. The whole of this increase cannot be viewed as a disturbance, since something must be allowed for normal growth, but the disturbance element in the increase could hardly be set at less than 1 billion dollars.

Though not the largest component statistically, the growth of U.S. direct investment in Western Europe is one of the most striking features of the increased outflow of U.S. capital. The spread of American manufacturing activity abroad is sometimes looked upon as another sign of inflated costs in the United States. Differences in production costs are doubtless an important consideration in many decisions to locate or expand manufacturing capacity abroad. It would be difficult to judge, however, to what extent these decisions reflect the emergence of new cost disparities and to what extent they manifest only a greater readiness to respond to long-standing differences in comparative advantages normal to international specialization.

Important deterrents to this response have been removed with the increase in strength of European currencies and with the equalization of war risks in the nuclear age. At the same time, positive inducements have been added by the rapid growth of other industrial countries and the creation of new preferential trading areas in the Common Market and the European Free Trade Association.

It is significant, moreover, how much of the new American manufacturing capacity being established in Western Europe is owned or managed through subsidiaries in Switzerland—an indication of the extraordinary fiscal inducements which such "tax havens" provide even with respect to investment outside their own boundaries [2]. The opportunities thus afforded for the reinvestment of foreign earnings without exposure to the high U.S. corporate income tax are

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[1] This statement has, of course, no bearing one way or the other on the possibilities or consequences of reductions in these outlays as a means of adjustment.

bound to look increasingly attractive to companies and stockholders interested in "growth situations" rather than current income.[2] The balance-of-payments effects include not only, and perhaps not so much, the stimulus to fresh capital outflow from the United States and to the deferral of income remittances but also, and perhaps much more important, the preference thereby created in favor of American plant expansion abroad to the possible detriment of future U.S. foreign trade.

Experience is still too recent, however, to permit a judgment as to how far these fiscal advantages may tip the scales in favor of the expansion of American operations abroad or as to how and how much this expansion may affect the U.S. trade and payments position.

III. *Past and Prospective Adjustments*

The argument up to this point is that, apart from cyclical and other short-term fluctuations, the U.S. has experienced during the last six years a number of more basic structural disturbances in its balance of payments. The adverse impact of these disturbances appears to have been in the order of 3 billion dollars on goods and services account and at least 1 billion on private long-term capital account. In addition to these new disturbances, the deficit existing in the base period would now have grown to 2 billion dollars if total receipts had risen no faster than total payments. The sum of these figures, about 6 billion dollars, is equal to 30 per cent of total receipts from abroad in 1953-55. This is a rough measure of the adjustment problem which has accumulated in a short span of years. It is what the U.S. has needed to make up in addition to covering a normal growth in payments for imports and other purposes.

If the levels of trade prevailing in recent months can be taken as a guide, the greater part of the adjustment needed would seem to have been made. The deficit on recorded transactions, exclusive of U.S. private short-term capital, averaged out to an annual rate of 1.4 billion dollars in the second and third quarters of 1960, or about the same as in 1953-55. In other words, the specific disturbances identified above have been offset by the slow growth in the bulk of U.S. imports and the fast growth in the bulk of U.S. exports, as observed in the discussion of Tables 2 and 3. These differences are clearly related to the slower and more erratic expansion of general economic activity in the United States than in other industrial countries during

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[2] An illustration in a study recently published by the American Management Association shows how, because of these tax facilities abroad, the reinvestment of foreign earnings can provide, over a three-year period, "roughly double the rate of profit accumulation for reinvestment possible under domestic tax schedules" [3, p. 34].

the period. It may also be that, apart from the particular items treated as disturbances, the income elasticity of U.S. demand for imports is lower than the income elasticity of foreign demand for U.S. exports.

The question arises, however, as to how much reliance can be placed on the levels and relationships reached in recent months. Exports include some obviously vulnerable items: deliveries of jet aircraft are due to fall off in the latter part of 1961; steel exports may decline because of growing foreign capacity for the qualities purchased here; and exports of nonferrous metals may have been higher for temporary reasons than can be sustained.

But some of the imports which have grown most rapidly in recent years also show little prospect of further growth in the near future: petroleum because of slower market expansion and import controls, automobiles because of the competitive response by Detroit, and meat because of the increase in domestic supplies after several years of shortage.

If these items which may now cease to grow or even contract are eliminated from both sides, the remaining products which have risen by more than one-third in the last six years still make up two-thirds of exports compared with only one-third of imports. These figures, though suggesting a generally stronger composition of exports than of imports, are again affected by differences in over-all growth rates at home and abroad. What happens if the economic tempo slows down in Western Europe and Japan and picks up in the United States? This is too large a question to be adequately explored in this paper, but several points may be suggested for consideration.

First, some moderation in the pace of expansion in other industrial countries seems inevitable and may already be visible. It makes a great deal of difference, however, whether this moderation results from a weakening of demand or from limitations of capacity. Of the first there is little sign except perhaps in the United Kingdom: the more general problem has been to keep demands from becoming excessive. The second, however, is clearly evident in chronic labor shortages, heavy demands for capital goods, and lengthening delivery dates by foreign manufacturers. A slowing down in growth for these reasons does not diminish but rather fortifies prospects for U.S. exports both to other industrial countries and to third markets.

Second, boom conditions in Western Europe and Japan are not typical of foreign markets elsewhere. There has been little increase in the last six years in U.S. exports to Canada, Latin America, and other countries dependent on exports of primary products. Their near stagnation is in turn related in some measure to the weakness of

U.S. demand for their products and, therewith, to the depressing effect on the prices which they receive for their exports generally. The situation in these countries could also pose a certain threat to continuation of economic expansion even in other industrial countries, as suggested by the recent weakness in British exports. This is why a slowing down or contraction of economic activity, though a possible means of balance-of-payments adjustment for a small country, is scarcely feasible for the United States: whatever gains there may be from restraining imports are likely to be wiped out by unfavorable repercussions on other countries and on their ability to buy from the United States.

The third point, therefore, is that, under these circumstances, a renewal of growth in the United States would not necessarily cause any net deterioration in the balance of payments. Though imports of raw materials would presumably rise, this should impart at least some strengthening to world commodity prices, improve the total export earnings of the primary producing countries, and remove some of the terms of trade advantage from which European balances of payments have so handsomely benefited since 1957. One might also expect that a significantly greater elasticity of supply in the United States would give it something of an edge, by and large, over its competitors in meeting the increased import demands of the primary producing countries. Nor need the benefits to the balance of payments from faster growth here be limited to the trade account. The effects should also extend to earnings from direct investments abroad in industries producing industrial materials. Perhaps even more important, a fuller utilization of capacity and more assured growth prospects in the United States should provide a better counterattraction to direct investment in manufacturing capacity in other industrial countries, and a rise in working capital needs here should reduce the incentive to employ idle corporate funds abroad.

IV. Concluding Remarks

This analysis has sought to explain the U.S. balance-of-payments deficits of recent years as the collective impact of a number of specific disturbances. Some of these have proved to be temporary, and others, on which the analysis is concentrated, are of a more structural and continuing nature.

The term "disturbances" has been used in a neutral sense to identify significant changes tending to reduce receipts or to increase payments. In a broader view, some of these disturbances may be considered undesirable—for instance, special obstacles to U.S. export competition and tax disparities unduly favoring foreign over domestic

investment. Other disturbances may be regarded as beneficial. Thus, the large increases registered in certain imports, though disturbing to particular sectors of the economy as well as to the balance of payments, have tended to strengthen international specialization, increase consumer satisfaction, restrain price increases, and stimulate innovation and modernization in American industry. These substantial benefits would be lost if the response to increased imports were to become protective rather than competitive in nature.

In a dynamic situation there are always some transactions which rise more rapidly, or less rapidly, than others. It may therefore appear somewhat arbitrary, even if consistent with theoretical precepts, to isolate as disturbances those shifts tending tb produce exceptionally large increases in payments' or shortfalls in receipts. The rationale of this procedure lies in the extent to which the period considered appears to have been marked by a one-sided concentration of structural shifts having adverse effects on the balance of payments, with the result that more aggregative methods of analysis could not, it would seem, satisfactorily explain the course of the balance of payments without making explicit provision for the intrusion of these important new elements.

It may nevertheless be objected that the shifts here treated as disturbances would have been more promptly and fully offset by other adjustments and lost from view, in a balance-of-payments perspective, if the U.S. economy had been more flexible and competitive. Stated in this way, however, the question again becomes one of judgment as to the relative severity of the disturbances experienced and the period of time reasonably required for appropriate adjustments to be made.

In consideration of the size of the adjustments needed, it would appear that encouraging progress has been made. The over-all export performance in particular looks impressive in relation to the barriers which have existed, and many of which still exist, against U.S. goods, and in relation to the depressed conditions prevailing in Canada, Latin America, and other nonindustrial countries which in 1953-55 took two-thirds of total U.S. exports.

It will be noted that the term "adjustments" has been used rather loosely to embrace the effects of differences in growth rates and in income elasticities of demand as well as such structural shifts of a favorable nature as have occurred. No attempt has been made to trace or impute a general mechanism of adjustment, whether automatic or policy-determined. Both types of responses may have affected to some extent the general pace of economic activity and the state of supply and demand at home and abroad, but have probably done little more

than reinforce other factors making for faster growth in other industrial countries than in the United States. Nor has any attempt been made to assess the effects of specific policy measures addressed directly to the balance of payments. These effects seem scarcely visible so far, except to a limited extent in the relaxation of foreign discrimination against U.S. exports.

In the large home oriented economy of the United States, however, it is perhaps to be expected that policies, whether general or specific, would intervene to affect the balance of payments only at the margin when required to prevent the interplay of other forces from shifting the balance too far in one direction or the other. At present, the margin is small enough—apart from short-term capital flows—to be closed by a further lead of only a few percentage points in the rise of receipts over that of payments. The task confronting policy is to ensure that the margin shifts in the right direction. Within this dimension, it should be possible to rely on measures which are consistent with other policy objectives at home and abroad and to reject solutions conflicting with these objectives.

There remains, however, the less basic and yet more urgent and vexing problem of preventing short-term capital outflows from continuing in their recent magnitude. In a world of large liquid assets and ready convertibility, this problem is likely to prove recurrent, for the dollar and for other currencies, at times when cyclical strains develop and especially when such strains are superimposed on other disturbances. Beyond the immediate and delicate problem of contriving a set of policies to reconcile the present internal and external requirements of the economy, there is the further task of improving policy instruments and institutions so as to prevent, or counter, future problems of this nature more effectively.

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UNBALANCED INTERNATIONAL ACCOUNTS: DIAGNOSIS AND THERAPY

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An imbalance in a country's international accounts means a persistent unintended change in its net gold and liquid foreign exchange reserves. For practical reasons, this paper will consider imbalance only when it involves a decline rather than an increase in reserves.

Imbalance so defined is a purely monetary concept: a decline in a country's external liquidity. It does not necessarily imply a decline in the country's "real" wealth, abroad or at home.

Diagnosis and Therapy of Imbalance in General

Imbalance always reflects an excess of total expenditures over total receipts; but it may appear primarily in a country's current accounts or in its capital accounts. Imbalance in current accounts is, as a rule, due to one or more of the following causes: (1) Domestic inflationary pressure. (2) Events abroad that induce foreigners to reduce their payments to, or to increase their demand for payments from, the country involved. Such events include not only general deflationary pressures abroad but also shifts in foreign demand or supply schedules that affect international transactions. (3) The development of substantial disparities between domestic and foreign price levels. Such disparities are often, though not invariably, the aftermath of past domestic inflation or foreign deflation; they need not reflect current inflationary or deflationary pressures.

Imbalance in capital accounts may be due to persistent disparities in yield levels, fears of unusual capital losses, or hopes of unusual capital gains. Persistent disparities in yield levels in turn may reflect divergent rates of economic growth, divergent institutional arrangements, or divergent economic or monetary policies. Fears of unusual capital losses most frequently are due to the dangers of inflation and devaluation, of confiscatory taxation, or of expropriation without adequate compensation. Hopes of unusual capital gains may be due, among other factors, to expectations of currency appreciation abroad or to the effects of foreign protectionist policies, including the trade diversion effects of customs unions.

Whatever the cause of imbalance, therapy must involve either an increase in total receipts to match the increased expenditures, or a

reduction in total expenditures to match the reduced receipts, or both. In view of the close relations between domestic and international transactions in a free economy, it is in general neither necessary nor sufficient to confine therapy to actions directly influencing international receipts or expenditures. A direct reduction in foreign expenditures, for instance, would be ineffective if it were offset by a rise in domestic expenditures that would eventually spill over into the international sector.

Whether imbalance should be corrected by raising receipts or by reducing expenditures is less a problem of theory than of practicability. In the field of current transactions, for instance, raising receipts usually, though not invariably, requires an expansion of output to provide for the necessary increase in exports, while reducing expenditures usually involves a contraction of consumption or investment. In most cases, moreover, care must be taken to prevent an increase in receipts from being offset by a corresponding increase in expenditures, or a reduction in expenditures by a corresponding reduction in receipts.

In deciding whether to aim primarily at raising receipts or reducing expenditures, the following five conditioning factors will have to be considered:

1. The volume of the country's actual and potential reserves, including foreign credit lines and drawing rights on the International Monetary Fund. This volume determines whether the country may choose therapies that are expected to work in the long run rather than in the short run.

2. The degree of utilization of domestic resources. If resources are fully and effectively utilized, measures envisaging an expansion of output are unlikely to be successful.

3. The degree of flexibility in the domestic economy. Full utilization of all domestic resources of labor and capital is a rare event. However, resources that are not fully employed can be used for expanding output only if they can be shifted to those sectors in which there is actual excess demand or a potential for a rise in demand.

4. The degree of flexibility in the world economy. If a country's trading partners permit deflation in their economies or raise other obstacles to an increase in imports, the country trying to correct an imbalance will be unable to increase its receipts by expanding its exports. Similarly, if foreign countries keep their capital yield levels unusually high or stimulate hopes of currency appreciation, the country trying to correct an imbalance will find it difficult to stem the outflow of capital. It was the virtue of the classical gold standard mechanism that it led to expansion in the surplus countries, thereby

helping to correct the imbalance of the deficit country. Under present monetary standards, ways must be found to replace that mechanism by conscious international co-operation.

5. Other policy considerations compelling a country to modify measures that would be appropriate on purely economic grounds. Such considerations, for instance, may make it impossible for a country to reduce economically unjustified government expenditures, or to curb imports of goods and services or the outflow of capital, or to oppose foreign protectionist or discriminatory policies.

These elementary theoretical considerations will now be applied to the present imbalance in the international accounts of the United States.

Diagnosis of U.S. Imbalance, 1958-60

Since the last quarter of 1957, the United States has suffered from an unintended persistent decline in net reserves which, according to the methods of measurement most widely used, exceeded 3 billion dollars annually. Until mid-1960 the source of the imbalance was to be found in the current sector; since then the deficit has also reflected unusual capital movements.

It is true that the current balance as usually defined showed a deficit only during part of 1959, a year in which the cyclical situation —first boom and then steel strike in the United States, with some stagnation abroad—stimulated imports and hampered exports. However, in view of the international obligations of the United States and the functioning of its capital market, the United States can be said to be in workable international equilibrium only if its current balance shows, over the cycle as a whole, a surplus large enough to cover the normal capital outflow. From this point of view, even the current surplus of the second half of 1960, large as it was, was not large enough. Disregarding unusual capital movements, total U.S. international accounts were in approximate balance rather than in surplus, although the cyclical situation (stagnation in the United States and boom abroad) was stimulating exports and restraining imports.

Prior to mid-1960, the variations in net capital outflows were not so unusual as to contradict the diagnosis of imbalance in the current sector. In a world of convertible and relatively stable currencies, cyclical changes influence not only the current but also the capital balance. On current account the cyclically weak country tends to have a surplus and the cyclically strong country a deficit; but on capital account it is the cyclically weak country that tends to have a

deficit and the cyclically strong country a surplus. Yields both on fixed-interest and on equity investments as well as the prospects of capital gains on equity investments will be more attractive in the cyclically strong than in the cyclically weak country. To some extent, therefore, current and capital balances tend to move in opposite directions during the cycle.

As to causes of the imbalance in the current sector, there has been little if any evidence of absolute or even relative general inflation in the United States since 1957, the last year in which the U.S. balance was in equilibrium. For both fiscal years 1960 and 1961 the government budget was well balanced. Since 1958 the money supply has hardly increased. For many more years, average gross earnings of production workers in manufacturing have risen less than output, and neither the cost of living nor wholesale prices have increased appreciably faster than in most other industrial countries, including Germany. It is true that export price indices have risen faster than abroad, but this divergence may, at least in part, reflect differences in the composition of exports rather than in actual price movements.

While there has been no general deflation abroad, foreign demand and supply schedules seem to have changed. Foreign industrial countries have improved their competitive position by catching up with the United States in technology and marketing, and their exports are no longer hampered by an abnormally high domestic demand due to reconstruction needs and lack of financial discipline.

Moreover, a persistent disparity between price levels in the United States and at least some foreign industrial countries has perhaps been created by the devaluation wave of 1949, as Professor Hinshaw suggested two years ago. If this devaluation was indeed excessive in some countries, the resulting price disparity could have remained without effect on the balance of international payments during the earlier period of abnormally high domestic demand abroad. According to this interpretation, it would be futile to look for evidence of unfavorable price or cost shifts in the statistical data for more recent years.

In any case, the adverse factors cannot have been very large, since the imbalance in the current sector has been small in recent months. It is therefore not surprising that statistical evidence on the matter has been inconclusive.

The recent imbalance in capital accounts is even more difficult to diagnose, since it covers a very short period and only fragmentary statistical data are available. There has been a large interest-rate differential between the United States and some, though by no means

all, foreign industrial nations since early 1960. It is difficult to judge, however, whether this differential has been larger than can be explained on purely cyclical grounds, and whether the resulting short-term capital movements should thus be considered cyclical and self-reversing or persistent and requiring therapy.

Unusual capital flows to Europe have included short-term flows based on expectations of an appreciation of the German currency, and long-term flows connected with the European Common Market. The long-term flow has probably also been influenced by tax differentials, and especially the fact that most if not all European countries have shown much higher rates of economic growth than the United States, and therefore justify hopes of larger yields and capital gains, quite apart from problems of exchange adjustments and discriminatory protection.

Finally, a significant part of the capital flow to Europe seems to have been due to misgivings about the future of the dollar as an international currency. This diagnosis is based on three facts. First, about half of the recent over-all deficit has been due to an unusual shift in the balance of unrecorded transactions from surplus to deficit. Second, a sizable fraction of the capital outflow seems to have gone to countries such as Switzerland and the Netherlands, where interest rates are not higher than in the United States. Third, an unusually large proportion of transfers to foreigners has recently been in the form of gold purchases from the U.S. Treasury rather than of increases in foreign dollar holdings.

Therapy of U.S. Imbalance

In general, countries are anxious to avoid a depletion of their gold or foreign exchange reserves in order to be able to maintain their imports even if their receipts from exports and capital transactions temporarily drop below their international expenditures. As long as the U.S. dollar is accepted as an international means of payments, the United States need not worry on that score. Both the United States and the rest of the free world, however, do need to worry about maintaining the role of the dollar as an international means of payments. This role is based on world-wide confidence in the stable value of the dollar in terms of gold and of the currencies of other leading nations; and this confidence in turn is, at least in part, based on the expectation that U.S. gold reserves will remain adequate, both in absolute terms and in relation to U.S. actual and potential short-term liabilities to foreigners. For this reason, the imbalance in the international accounts of the United States needs therapy even though

it may reflect an exchange of investments abroad for gold and short-term dollar liabilities rather than an excess of current expenditures over receipts.

In fact, therapy must aim at more than at restoring the balance-of-payments position of the early fifties when the United States had a deficit averaging 1½ billion dollars annually without adverse effect on confidence in the U.S. dollar. At that time, the dollar was generally considered "scarce" abroad and an increase in foreign dollar holdings therefore advantageous and even necessary for the expansion of world trade. The increase in foreign gold and dollar holdings of about 13 billion dollars that occurred in the past three years has put an end to the international dollar "scarcity" and has given rise to a fear of a dollar glut. Therapy must therefore aim at eliminating rather than merely reducing the over-all deficit in the U.S. balance of payments.

This therapy must affect both current and capital transactions. During the past few years so much has been said about the elimination of a deficit on current account that few if any new ideas can be put forward. Since technological progress abroad seems to have played a role in creating the U.S. deficit, it should be obvious that the United States, in spite of its affluence, cannot afford to neglect the problem of continuing to raise its economic efficiency, while guarding against further increases in costs and prices. If it is correct to assume that price discrepancies also have resulted from excessive devaluation of one or two major foreign currencies, the United States should support efforts of those countries to remedy the situation.

Obviously, the United States should try to correct the current imbalance as much as possible by increasing exports rather than reducing imports, with the equally obvious proviso that economically unjustified payments, for instance, on government account, should be eliminated. However, this principle may be affected by three of the five conditioning factors previously discussed.

The first two of them need not detain us. The U.S. gold reserves still are large enough to permit as long a period of transition as can reasonably be expected. They are about twice as large as the short-term dollar holdings of foreign monetary authorities, who alone can demand conversion of their holdings into gold; and they can be further defended, if need be, by using other resources, such as drawing rights on the International Monetary Fund.

Second, an expansion of output is, unfortunately, not hampered by the full employment ceiling since much labor and capital have been unemployed for some time.

In contrast, economic flexibility needs to be increased both at home and abroad. The presence of rigidities in the U.S. economy is indicated by the persistence of serious unemployment side by side with continued increases in some prices and in wages. These rigidities have been attributed to various factors, such as monopolistic practices of labor unions or of big business. Perhaps more important than either of them is the increasing bureaucratization of business, as shown by the rapid rise in the white-collar staff of manufacturing enterprises. According to Professor Schultze's data, this increase has contributed more to raising the cost of manufactured products than increases in wage rates or in profit margins. Fiscal policy might thus have to be used to curb Parkinson's First Law in the business world.

However, bureaucratization is perhaps merely a symptom of deeper trouble, of a general rise in inertia and complacency, a subconscious protest against the restlessness of our times. Unless the United States overcomes this tendency, I fear that institutional changes will not suffice to prevent more active nations from outcompeting the United States, and its balance of payments will be confronted with the same kind of problems that some older countries have had to face ever since the first World War.

On the international scene, the case against foreign protectionist or discriminatory policies should be as obvious as the case against those features of regional organizations that would divert rather than create trade. U.S. therapy cannot be successful if major foreign countries with balance-of-payments surpluses pursue monetary or commercial policies that interfere with the required expansion of U.S. exports. The need for increased flexibility also furnishes another argument against attempts to correct the U.S. imbalance by imposing new rigidities, such as restrictions on imports or on the flow of capital.

The fifth conditioning factor—the influence of other policy considerations—is even more important. The most serious, if not the only serious, consequence of a persistent U.S. imbalance would be the decline in the international prestige of the dollar and the resulting threat to the international dollar standard. Therapies that would undermine that standard, such as an increase in the price of gold or other attempts at changing the established par value of the dollar, would thus be as bad as the disease.

Moreover, grave as a danger of a collapse of the dollar standard would be, it would not be as disastrous as threats to the preservation of the Atlantic Alliance or to the economic progress of less-developed areas. Therapies that might endanger the attainment of these goals are therefore out of the question. Changes in foreign trade, foreign economic aid,

and foreign military expenditures can be considered only insofar as they do not conflict with basic U.S. foreign policies, even though they may be desirable from a purely economic point of view. Policies requiring foreign aid to be spent on U.S. goods and services, for instance, are acceptable only because they put pressure on other industrial countries to finance their own exports to less-developed areas and in this way tend to increase rather than reduce the total funds available to those areas.

The imbalance on capital accounts might best be corrected by removing the causes of the unusual capital outflow. Insofar as the outflow is due to more rapid economic growth abroad, the obvious therapy is to accelerate growth at home. Insofar as it is due to tax considerations, a revision of fiscal policies and an attempt at equalizing tax rates in the major industrial countries may be in order.

Insofar as the outflow is due to the expectation of appreciation of a major foreign currency, the foreign country should be induced either to have done with that appreciation or to pursue policies that eliminate the need for appreciation. Insofar as it is due to fears of U.S. domestic policies that would endanger the par value of the U.S. dollar, the equally obvious therapy is to prove that these fears are unjustified by continuing to pursue monetary and fiscal policies consistent with long-run financial equilibrium.

A more difficult problem is posed by those capital flows that are due to interest-rate differentials. Under present conditions of currency convertibility and freedom of capital transactions, it is inevitable that a certain amount of interest-sensitive funds will take advantage of such differentials. Whenever these differentials are cyclical, the resulting flows are in principle reversible and therefore would seem not to require therapy. However, large cyclical flows, by their effect on U.S. gold reserves, may create apprehension or even panic before they reverse themselves, and thus lead to accelerated and persistent secondary flows.

This danger cannot be avoided by the actions of any single monetary authority. It would be self-defeating to prevent interest rates from falling when a country was on the verge of recession or stagnation; such a policy would merely hamper economic recovery and growth and thereby induce an outflow of long-term capital. The danger can be avoided, however, if all major countries decide to co-operate in neutralizing the effects of cyclical flows of short-term funds so as to avoid secondary flows. There are many ways in which co-operation could be accomplished, including the use of the International Monetary Fund or the Bank for International Settlements. Some interesting suggestions to this effect have been made recently, but this is not the place to argue their respective merits.

The preservation of the international dollar standard certainly is in the interest of the United States, not so much because of its rather modest advantages for the U.S. banking system as because of its importance for the cohesion and expansion of the international commercail and financial network. However, for the same reason it is as much, or more, in the interest of all other financial and commercial nations. These other nations have the right to expect that the United States will do its part to bring its international accounts into structural balance. The United States, however, has the right to expect that other nations in turn will do their part to help preserve the monetary standard of the free world.

THE ADEQUACY OF UNITED STATES GOLD RESERVES

By EDWARD M. BERNSTEIN

Before the European currency devaluations of 1949, the gold reserves of the United States amounted to 24.8 billion dollars and constituted 72 per cent of the monetary gold of all countries outside the Soviet bloc, but excluding gold held by international institutions. In the eleven years since then, U.S. gold reserves have fallen by 7 billion dollars. The disturbing aspect about the U.S. reserve position is not that the gold reserves are so small but that there is no assurance that the rapid decline of recent years (more than 5 billion dollars from 1958 to 1960) is coming to an end.

U.S. Reserve Position

It seems paradoxical that there should be any question of the adequacy of the reserves of a country holding 17,767 million dollars in gold, constituting (at the end of 1960) 47 per cent of the monetary gold of all countries outside the Soviet bloc. In fact, it is a reasonable question. The reserve position of the United States is comprised of a number of elements of which gold is the most important, but not the only one of importance. A comparison of the gold holdings of the United States with those of other countries exaggerates the relative strength of the U.S. reserve position.

Official institutions, banks, and private holders abroad now have over 19 billion dollars of short-term and liquid dollar claims. Foreign official holdings of dollars are convertible into gold; other holdings can be transferred to central banks and become convertible into gold. The dollar claims held by other countries add to the strength of their reserve position; and these obligations must be taken into account in assessing the reserve position of the United States.

The U.S. Treasury does not hold foreign exchange as reserves. However, U.S. banks do hold over 3 billion dollars of short-term foreign claims for themselves or for their clients. In many countries, but not in the United States, the foreign exchange held by banks is regarded as available to the monetary authorities. Even though they are not part of the reserves as generally defined, the short-term foreign claims held by U.S. banks are an element of strength in the U.S. reserve position.

It should also be noted that the U.S. government is a large creditor of foreign governments and official institutions. At the end of 1959, U.S. government credits and claims amounted to over 20 billion dollars, of

which 8.5 billions represent long-term debts of Western European countries. If necessary, it would be possible to expedite repayment of some of these claims by high-income countries with a strong balance of payments. Among the credits and claims that are of particular importance in assessing the U.S. reserve position is its credit in the International Monetary Fund. The Fund is an institution to which its members have subscribed capital in the form of gold and their national currencies to constitute a common reserve. The United States is a creditor of the Fund to the extent of nearly 1.6 billion dollars, representing its gold subscription and net sales of dollars by the Fund. While the United States has

PRINCIPAL FACTORS IN THE U.S. RESERVE POSITION
(IN BILLIONS OF DOLLARS)

Year end	Gold Reserves	Net Fund Position	Short-term Foreign Assets of U.S. Banks	Short-term Foreign Liabilities to				Other Foreign-owned U.S. Government Securities
				Official Holders	Banks	Others	Fund* (Gold)	
1950	$22.82	$1.45	$ .90	$3.88	$1.84	$1.40		$1.28
1951	22.87	1.48	.97	3.94	2.20	1.52		.61
1952	23.25	1.46	1.05	4.91	2.37	1.68		.90
1953	22.09	1.37	.90	5.85	2.39	1.78		.81
1954	21.79	1.19	1.38	6.98	2.36	1.80		.75
1955	21.75	1.04	1.55	7.29	2.65	1.78		1.31
1956	22.06	1.61	1.95	8.27	3.19	2.08	$.20	1.10
1957	22.86	1.98	2.20	7.91	3.47	2.25	.20	1.22
1958	20.58	1.96	2.54	8.66	3.52	2.43	.20	.98
1959	19.51	2.00	2.62	9.14	4.69	2.40	.50	1.50
1960	17.81	1.56	3.55	10.37	4.83	2.21	.80	1.42

* Fund holdings of U. S. Treasury bills purchased for its "gold investment" account. The noninterest-bearing notes of the U.S. Treasury are accounted for in the "Net Fund Position."
SOURCE: *International Financial Statistics.*

never drawn on the Fund, the Fund has invested 800 million dollars of its gold holdings in U.S. Treasury bills and this is actually a gold loan from the Fund.

The legal requirement that Federal Reserve banks hold gold certificates equal to 25 per cent of their liabilities on Federal Reserve notes and deposits is not really an element in the U.S. reserve position. The time is long past when the monetary authorities could regard the gold reserve as an objective measure of the proper quantity of money. Instead, a gold reserve requirement may hamper the monetary authorities in making policy. When reserves are near the legal minimum, there may be pressure to make money too tight. Furthermore, it is sometimes thought that the required gold reserve (now about 12 billion dollars) is untouchable. Actually, the reserve can be used under present legislation

to meet any outflow of gold. Nevertheless, it may be helpful, as Mr. Reierson has suggested, to abolish gold reserve requirements entirely, retaining the obligation to maintain the present gold value of the dollar.

U.S. Reserve Needs

The adequacy of U.S. gold reserves cannot be determined merely by adding and subtracting the various elements in the reserve position. It is rather a matter of judgment regarding the need for reserves that may emerge in the future. An adequate reserve for the United States is one which would enable it to meet balance-of-payments deficits and conversions of foreign dollar claims without necessitating extreme measures to deflate the domestic economy or to impose restrictions on international trade and payments. In short, an adequate reserve is one which would give the United States time to restore its balance of payments, not obviate the need to do so.

One of the common tests of the adequacy of reserves is the ratio of gross reserves to imports. The concept underlying this measure of adequacy is that a country must continue the flow of imports to maintain consumption and production. At present, U.S. gold reserves are equal to 110 per cent of the value of 1960 imports—still quite high by comparison with other countries. It is not a convincing demonstration of the adequacy of U.S. reserves, however, to say that the ratio of reserves to imports is substantially higher than in other countries and about two and a half times as high as the average for the rest of the world.

The United States has a special need for large reserves that is not shared by any other country except the United Kingdom. The gold reserves of the United States must be adequate, not only to meet deficits on account of trade and other payments, but also the conversion of official dollar holdings into gold and the transfer of private funds (foreign and domestic) to other financial centers where they may be acquired by official institutions and be converted into gold. At present, U.S. gold reserves are about equal to the total of foreign short-term and liquid dollar claims. The ratio of U.K. reserves to sterling balances is under 35 per cent. Because about three-fourths of the sterling balances are held by sterling area countries, the United Kingdom is not as exposed as the United States to the risks of a reserve center. In any case, British reserves are much too small and cannot be the standard for determining the adequacy of U.S. reserves.

A reserve center needs generous reserves to give it freedom to counteract recessions without fear of the adverse effect of fiscal and credit policy on its reserve position. The United States has not found it necessary to terminate an expansion merely to protect the balance of payments. Nor has the Federal Reserve found it necessary to be unduly

cautious in easing credit in a recession. It is worth noting, however, that the yield on three-month Treasury bills has not fallen below 2 per cent in this recession, although it went down to nearly ½ per cent in May, 1958. Even with bill yields of more than 2 per cent, there has been an enormous outflow of short-term funds and large net sales of gold.

Obviously, the adequacy of U.S. gold reserves cannot be judged independently of the balance-of-payments prospects. The willingness of other countries to hold dollars depends upon their confidence that the United States will maintain the present gold value of the dollar and continue the free transfer and convertibility of foreign dollar holdings. Confidence in the dollar depends much more on the strength of the U.S. balance of payments than on the precise magnitude of U.S. gold reserves. Foreign confidence in the dollar would be much greater with a surplus in U.S. payments, even if U.S. gold reserves were only 15 billion dollars, than it is now with over 17 billions of gold reserves, but with the balance of payments in persistent deficit.

The gold reserves of the United States are still large enough to meet deficits in the balance of payments for many years. They are large enough to finance any probable conversion of official dollar balances into gold. Nevertheless, the United States must now be concerned with the effects of domestic policy on the balance of payments. We have not lost the freedom in policy making that our enormous reserves gave us in the past, but we shall lose it if payments deficits are allowed to continue. The best way to assure the adequacy of U.S. gold reserves would be to strengthen the payments position of the United States.

Growth of World Reserves

The maintenance of an adequate level of gold reserves in the United States ultimately depends upon an adequate growth in world monetary reserves—say, at about the same rate as the growth in world payments. The gross official reserves of all countries outside the Soviet bloc now amount to about 59 billion dollars in gold and foreign exchange. This does not include private foreign exchange holdings; nor does it include the resources of the International Monetary Fund. Between 1950 and 1960, nearly all of the large industrial countries succeeded in rebuilding their reserves, largely by acquiring gold and dollars from the United States. The reserves of the United Kingdom and of a few other large trading countries are still too small. Many of the low-income countries are unable or unwilling to invest large resources in holding monetary reserves. Despite these exceptions, it may be concluded that, with the resources available from the Fund, the world level of reserves is quite adequate for the present.

It is unlikely, however, that aggregate monetary reserves can grow at

the rate needed by an expanding world economy. In the ten years from 1951 to 1960, monetary gold outside the Soviet bloc increased by 5.5 billion dollars, an average annual increase of less than 1.5 per cent of the monetary gold stock (including gold held by the Fund) and about 1 per cent of gross monetary reserves. In this same period, foreign official holdings of short-term dollar assets (excluding the Fund) increased by over 6 billion dollars and other foreign holdings of dollars increased by 4.3 billion. Thus, most of the increase in aggregate gold and foreign exchange reserves from 1951 to 1960 is attributable to the increase in short-term dollar claims on the United States.

The world is approaching the practical limit of its capacity to increase monetary reserves through additional holdings of the reserve currencies. At the end of June, 1960, foreign sterling balances were only 350 million dollars larger than they had been at the end of 1950. Foreign dollar balances are still rising, but it is uncertain whether they can continue to increase very much more. From its own point of view, the United States should be unwilling to see a substantial increase in foreign-held dollar balances unless it is matched by an almost equal increase in U.S. gold reserves.

As the world's need for additions to monetary reserves will be much greater than the normal increase in the monetary gold stock, it will have to be met in some other way. A number of distinguished economists believe that the addition to reserves should be in the form of gold, and they have advocated a uniform increase in the price of gold in terms of all currencies. This would add substantially to the money value of present gold reserves and of future increments from new production, private hoards, and gold sales of the Soviet Union. Such a drastic measure is unnecessary and undesirable. It would nullify the postwar efforts to establish monetary stability and orderly currency arrangements. Furthermore, it would be an act of bad faith toward those countries that have held reserves in the form of dollars and sterling.

The best way to deal with future needs for additional reserves is through the International Monetary Fund. The quotas of the members of the Fund can be increased from time to time. More important, however, the Fund must give its members complete assurance that they will be able to regard their quotas in the Fund as freely available to them. The quotas of all the members of the Fund should be integrated with their working reserves, not set apart as a second line of reserves for extreme cases or emergencies. Members should be able to use their annual quotas without formality to meet ordinary fluctuations in the balance of payments.

The United States has a net creditor position of over 1.5 billion dollars in the Fund. The Treasury should use these resources whenever it

has a payments deficit or an outflow of gold. The Federal Reserve Bank of New York could arrange to sell such currencies in the exchange market whenever there is pressure on the dollar. The premium from the sale of foreign exchange at such a time would more than offset the transactions charge of the Fund. It has been said that U.S. drawings on the Fund would undermine confidence in the dollar. Foreign monetary authorities and exchange traders are too well informed to be influenced in their judgment regarding the dollar by whether or not the United States draws other currencies from the Fund. It is irrational to borrow 800 million dollars in gold from the Fund, at Treasury bill rates, when the United States could withdraw some of the resources it has contributed to the Fund without any interest cost.

Problem of the Reserve Centers

Excluding the United States and the United Kingdom, about 55 per cent of the official reserves of all countries, outside the Soviet bloc, are held in foreign exchange—essentially dollars and sterling. The widespread dependence on dollars and sterling as monetary reserves involves great responsibilities for the reserve centers. For the United States, particularly, with 19 billion dollars of foreign dollar claims, with complete freedom to transfer funds abroad, there is the danger that a deep depression in this country or a political crisis in any country could lead to a serious drain on U.S. gold reserves. If the world economy is to avoid the disruption that would inevitably attend a large-scale movement away from dollars or sterling, it is essential to assure foreign governments that their holdings of these currencies will always be available for free use as reserves at their present gold value.

Professor Triffin has taken the view that it is no longer feasible to continue the present system of using dollars and sterling as major constituents of monetary reserves. He has proposed a complete reformation of the International Monetary Fund to convert it into an international central bank in which members would hold part of their reserves of gold and foreign exchange. Thus, by depositing dollars and sterling in the Fund, member countries would be assured that the gold value of these currencies would be maintained. And Triffin would have all other countries accept deposits in the Fund for conversion into their currencies for use in international payments. The reformed Fund would provide for the future reserve needs of an expanding world economy through its power to create reserve credit. The United States and the United Kingdom would be relieved of the risks of large and sudden drains on their gold reserves by ceasing to be reserve centers. The Triffin plan deserves serious consideration by the United States and other members of the Fund.

My own approach to the problem of the reserve centers, however, is quite different. The reserve centers perform an important function for the world economy by financing international trade and investment. If dollars and sterling were no longer held as reserves, the capacity of the reserve centers to provide finance for the world economy would be impaired. The best solution to the risks inherent in widespread holding of dollars and sterling is not to abolish the use of these currencies as international reserves but to enable the United States and the United Kingdom to meet their responsibilities as reserve centers.

The basis for protecting the reserve centers from a flight from dollars or sterling into gold already exists in the statutes of the Fund. Under Article VIII-4 of the Fund Agreement, a member is obligated to convert official balances of its currency into gold or into the currency of the country requesting the conversion. There is no reason why the United States should not make use of this provision whenever necessary. It would not involve a departure from the traditional policy of selling gold to foreign central banks for the settlement of international transactions. It would provide an essential safeguard against a flight from dollars into official hoards of gold.

To use such a provision, the reserve centers must have access to rather large amounts of the leading currencies. The Fund holds about 2.4 billion dollars in gold, an additional 800 million in its gold investment account in the form of U.S. Treasury bills and about 6.6 billions in the currencies of the great trading countries. These resources are large enough to enable the United States and the United Kingdom, or other countries confronted with a major outflow of funds, to meet conversion requirements by drawing on the Fund. The practical difficulty is that these resources may also be needed for transactions with other members to meet their balance-of-payments deficits on current account.

For this reason, it would be desirable to have the Fund enter into arrangements with all of the large holders of reserves under which their governments or central banks would undertake to lend the Fund stated amounts of their own currencies, at the Fund's request, in return for an interest-bearing note of the Fund with a gold guarantee. The note would have a definite maturity, say four years, but would be repaid sooner under specified conditions. The Fund would call on a member to take up all or part of its agreed subscription to these notes whenever another member needs that currency to meet a major outflow of funds or is presented with a demand for conversion of large balances by that country.

The operations under such a plan would be comparatively simple. Suppose there is a large outflow of dollars from the United States to the United Kingdom, Germany, and the Netherlands, and the central banks of these countries present the dollars for conversion into gold. The Fund

would borrow an equivalent amount in sterling, marks, and guilders from these countries. The currencies would be lent by the Fund to the United States which would offer the sterling to the Bank of England, the marks to the Bundesbank, and the guilders to the Netherlands Bank for conversion of their dollar balances. After a year or two, if there is an outflow of funds from the United Kingdom to the United States, the United Kingdom could present the Fund's note for redemption. A country lending its currency to the Fund would be able to secure immediate repayment when it needs other currencies to convert foreign-held balances.

To avoid a drain on the present resources of the Fund, it might be preferable to conduct these transactions through a subsidiary institution, say a Reserve Settlement Account, managed by the Fund. The transactions of the Reserve Settlement Account would be outside the quotas of the Fund and would be on terms and conditions agreed with each member. The Reserve Settlement Account could be given other powers, say, to accept deposits of gold, to hold gold on earmark, to borrow and lend gold, and to buy and sell gold. In fact, the Reserve Settlement Account could become a convenient means through which reserve settlements are made without the transfer of gold.

This proposal envisages the wider use of currencies along with gold in the conversion of foreign-held balances, a right already open to all members of the Fund. The resources that would be available to the Reserve Settlement Account would be enormous if the large holders of reserves, such as the United States, Canada, the United Kingdom, France, Germany, Italy, Netherlands, Belgium, and Japan, were to undertake to finance their own demands for conversion of other currencies. A Reserve Settlement Account that finances the conversion of currencies is a logical complement to an International Monetary Fund that finances payments deficits on current account.

DISCUSSION

JAMES BURTLE: My comments are directed to Hal Lary's paper. He has succeeded admirably in putting the many diverse elements of the United States balance of payments into a comprehensive and yet manageable form. In his analysis of the United States trade account, the main influences that are perhaps amenable to economic analysis are income effects and price effects. Insofar as income effects are concerned, I would stress somewhat more than Lary that 1960 was an unusual year. For the first time since the Korean war year of 1952, United States imports declined while at the same time there was a rise in United States exports. Imports, especially of raw materials, should be expected to rise again as soon as the economy resumes a more normal rate of growth. The present level of exports, on the other hand, is probably a reflection of abnormally high levels of economic growth abroad. In Europe, the 1960 rate of increase in industrial production is roughly estimated at 10 per cent, which is the most rapid increase since 1955. In Japan, the rate of increase at about 20 per cent is the highest since 1956. Insofar as income effects are concerned, I would suggest that in a year more "normal" than 1960, the United States trade account would show a somewhat lower level of exports and a higher level of imports. Lary believes, on the other hand, that the demands of Europe and Japan for capital equipment, especially laborsaving machinery, may maintain United States exports at a high level. I would doubt whether the effect of laborsaving machinery would be of crucial importance to United States exports. If the rate of growth slows down in Europe, this is likely, via the acceleration principle, to result in some excess capacity in its own facilities for producing capital goods. To verify Lary's point we would also want to know the magnitude of imported machinery required for labor saving. A good deal of labor saving results from new buildings designed for better lay-out and from changes in organization. My guess is that the current boom in demand for capital goods from the United States is simply from broadening the capital base rather than from laborsaving machinery per se.

Another interesting suggestion of Lary is that a faster rate of growth would make a positive contribution to the United States balance of payments because, if primary product prices were raised as a result of higher United States purchases, there would be a rise in earnings and more imports from the United States by countries exporting these products. This type of secondary repercussion could occur in theory. But I doubt if the facts make it likely. Among South American export commodities, for example, petroleum, lead, zinc, and meat are limited by restrictions in the United States. Coffee, copper, and bananas have no crucial restrictions on entry into the United States, but I am inclined to believe that elasticity of supply is strong enough in these commodities that substantial price increases are likely to be followed within a relatively short time by increased output and again lower prices. Bananas require a relatively short time to bring into production. Higher United States demand for coffee is likely to result in easing export quotas under the world coffee agreement. If copper

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prices increase substantially, I would expect to see more expansion of low-cost copper mines either in South America or in Africa. I believe that Lary's argument was strengthened, however, when he suggested that increased export earnings of primary producers would be an inducement to additional domestic investment in these countries. Higher investment would in turn result in higher incomes and higher imports from the United States.

A higher rate of economic growth is also suggested to have the effect of raising United States profits and thus encouraging an inflow of capital. In connection with United States investment abroad, Lary suggests that unless so-called "tax haven" countries discontinue offering special inducements to investment, the United States should apply the standard corporate tax to net earnings of American subsidiaries abroad whether or not dividends are actually paid out to American citizens. This is, of course, as much a complicated legal problem as an economic problem, but I would note that such a policy is not followed by other countries with investment abroad. Some countries, in fact, do not tax earnings paid out as dividends if foreign taxes have been paid. Thus, the policy that Lary suggests would put United States firms abroad at a marked competitive disadvantage. If an American firm in Thailand, for example, was competing with a British firm, the American firm would be obliged to pay taxes each year out of all consolidated earnings. The British firm, on the other hand, would be able to accumulate and reinvest earnings and pay taxes only when it remitted dividends.

I am somewhat less optimistic than Hal Lary on income effects in relation to the United States balance of payments. I would, on the other hand, take the view that the outlook for the effects of prices on the United States balance of payments is likely to be favorable, although income effects may be unfavorable. Because of the apparent rise in costs in Europe, I do not see any compelling reason why United States exports should be priced out of foreign markets. At this point, however, I must express some feeling that Lary's method is insufficiently aggregative and perhaps gives too much attention to special situations. If policy-makers and businessmen have to make decisions on the outlook for the United States balance of payments, they will hope to find some thread of events that runs through past experience and which may give us some guidance for the future. Sometimes, of course, the search for generalization can result in analytical models that fail because of lack of concern with special situations. There is, on the other hand, a danger of viewing every situation too much in isolation. Two rough hypotheses about the apparent price behavior of our foreign competitors may be useful at this point. First, although European and Japanese costs are on the average lower than ours, such costs show our competitors' advantages incompletely because most output abroad is to some extent institutionally rationed, to achieve a high level of sales in domestic markets before a search is made for foreign markets. Thus differences between United States costs and foreign costs may persist for a long time before foreign products break into the United States market. Second, our competitors do not break into markets in the United States on a broad front because the United States requires notoriously high marketing and distribution costs. In a particular year, only a few foreign industries may feel strong enough to make the

push into the United States market. In 1959, as we well know, the push was in autos. This year steel and nonferrous metal products made big gains.

What is a perhaps dangerous stress on special situations may be to regard each of such developments as separate influences. They may all have price factors in common, though high overhead selling costs make it unlikely that there will be many substantial penetrations of the United States markets in any one year. What Lary calls a greater availability of autos in 1959 might be followed by a greater availability of machine tools or appliances some time in the sixties. All of these situations might have an element of price competition in common and it would be a pity to regard them as purely historical events without economic content simply because we cannot analyze the essentially institutional factors which may determine what will be the next commodity that will break into the United States market.

It is, of course, true that penetration of the United States market is affected, not only by price, but by quality differences as well. I would suggest, however, that price differences are not bad as proxy variables for many changes in technology and styling that have enabled important spearheads into the United States markets. It seems to me that firms abroad that have led the way in cost cutting have also led in innovating, styling, and effective advertising while United States firms that have been unsuccessful at cutting costs also have had poor records in innovation, styling, and sales promotion.

These considerations lead me to the admittedly hazardous method of using ratios of foreign to United States export price indices as a prime indicator of the United States competitive position. These indices involve, of course, all kinds of technical problems of weighting. There is, however, a difficulty that is perhaps greater than the technical problem of constructing indices of relative export prices. In some countries, the United States share of total imports tends over business cycles to vary directly with the total volume of imports. When imports are high, the share of imports from the United States is high; when imports are low, the share of imports from the United States is low. The United States share probably tends to move with imports because in periods of prosperity and higher imports, there are greater demands for investment goods and luxury products—which come to a greater extent from the United States—and less discrimination against imports from the United States. Between 1957 and 1958, for example, total imports, roughly corrected for price changes, fell 9 per cent in Chile, 17 per cent in Colombia, and 10 per cent in Venezuela. Corresponding declines in the United States share were from 49 per cent to 41 per cent in Chile, 58 per cent to 54 per cent in Colombia, and 63 per cent to 58 per cent in Venezuela. The U.S. share in these countries appears to be affected by two factors: the total volume of imports and the ratio of export prices of our competitors to United States export prices. This leads to a difficult problem of relating the United States share to two possibly intracorrelated variables.

I have, however, made an experiment—and this should be considered illustrative rather than a firmly drawn conclusion—of relating the United States share of imports into Colombia and Venezuela to ratios of a weighted average of European export price indices to a United States unit value index of ex-

ports for 1952-56. These were years in which there were no violent cyclical fluctuations in imports of these countries so the price effect rather than the income effect may have predominated in affecting the United States share. Between 1952 and 1953, a seven point drop in the price ratio was matched by a four point drop in the United States share in Colombia and a six point drop in the United States share in Venezuela. Between 1953 and 1954, a one point drop in the price ratio was matched by a one point drop in the United States share in Colombia and a three point drop in the United States share in Venezuela. Between 1954 and 1955, there was a two point drop in each series. The previous relationship did not hold well in 1955-56, perhaps because of imports of oil equipment which comes mainly from the United States.

PETER B. KENEN: I shall address most of my remarks to Dr. Furth's excellent paper and especially to his diagnosis and therapy for the payments position. I will start, however, by quibbling with one point he makes in his analytical preface.

Furth's definition of a payments deficit (an unintended decline in net reserves) needs amendment if it is to be applicable to a reserve-currency country like the United States. An American deficit *is* matched by a decline of United States net reserves, but the urgency of action against such a deficit will depend upon the way that net cash declines. If the decline involves a gold loss (a reduction of assets) rather than an increase of foreigners' dollar balances (an increase of liabilities), it may need more rapid correction than would otherwise be necessary. It was, I think, because other countries chose to build up their dollar balances that we were able to run a payments deficit from 1950 through 1956 without feeling obliged to contemplate remedial action. We may now be suffering from our failure to exploit that prolonged opportunity to examine and overhaul our international position.

Let me begin my review of Furth's diagnosis by seconding his distinction between the "normal" and "extraordinary" parts of the American deficit. Although we should not always act against those items that have produced the deficit but should sometimes offset them by acting on others, we should never correct an extraordinary deficit by operating on the normal balance or combat a normal deficit as though it were extraordinary. Furth, then, has produced a vital distinction for the analysis of the American position.

I am slightly more optimistic than he is as regards this extraordinary deficit —the froth of short-term capital movements that has capped the normal deficit. I doubt that we have yet been witness to a flight from the dollar, for most of the recent short-term capital outflow can be sufficiently explained by interest arbitrage of one sort or another. The adverse shift in "errors and omissions," to which Furth alludes, may represent interest arbitrage; it may be due to a change in the methods of financing United States foreign trade caused by an interest-rate differential. Moreover, our data on explicit short-term capital movements are woefully inadequate, and some of the transactions that enter as "errors and omissions" may be identical in form and motive to those that are caught in the statisticians' net. Nor should we regard the large gold outflow of 1960 as decisive evidence of a run on the dollar. A

deficit on account of interest arbitrage is quite apt to cause gold losses, for most of the dollars that leave this country in search of higher yields will find their way into the hands of gold-holding central banks.

I would not exclude the possibility of a run on the dollar—present or imminent. But the evidence Dr. Furth has offered here is not, I think, conclusive. And provided there is no sharp deterioration in foreign confidence during the next few months, we should see a reflux of short-term capital during 1961. The next year may witness a change in interest rates that will bring home American money and attract foreign funds to New York.

While I am less worried about the extraordinary deficit I share Furth's concern about the prospective normal deficit. We must anticipate a recrudescence of the deficit on current *cum* long-term capital account when the cyclical position changes again. U.S. exports are probably larger than they would be were Europe stagnant, and imports are smaller than they would be were the United States prosperous. Furth has rightly observed that the outflow of long-term capital could slacken with the onset of domestic recovery. But the cyclical improvement on capital account may not fully offset the prospective deterioration on current account. The new Administration, moreover, may increase foreign aid and relax some of the restrictions imposed by its predecessor. These steps would also enlarge the normal deficit.

Furth warns that the dollar will be suspect until this normal deficit is completely closed. I would go further, to argue that the United States can never again plan to run regular deficits as it did in the early fifties. Mr. Bernstein has said—and I would agree—that the world may be near the limit of its tolerance for dollar assets. Foreigners may not want to accept more even after we have proven our capacity to control our payments position.

I would also question the adequacy of American reserves. Although our gold reserves are twice as large as our liabilities to foreign governments, a flight from the dollar would shift private dollar balances into official hands, and they might then be converted into gold. The Treasury's gold stock may not be large enough to cope with the claims that could be made upon it, and remedial action may be even more urgent than Furth implies.

Despite my misgivings on this point, I too would urge "expansion" as a solution to the payments problem. Restraints on the American economy or, directly, on payments to foreigners would end the imbalance at the expense of countries than can ill-afford the attendant strains. The United States must therefore resume negotiations with the surplus countries, both with respect to defense and foreign aid and with respect to ordinary financial policy. Furth has said there is no evidence of general deflation abroad. Yet the financial policies of some surplus countries are insufficiently exuberant. They could frustrate our efforts to end the present imbalance.

To ask for help from the surplus countries is not to confess the bankruptcy of American policy. It is rather to insist that balance be restored without disrupting world trade or capital movements. The United States can solve its problem unaided, but only by measures that would damage world trade and might unravel the fabric of commercial and financial arrangements that has been so laboriously woven since World War II.

Let me now revert to the distinction between deficits financed by gold losses and deficits financed by increases in foreigners' dollar holdings. Both impair the American reserve position. But a gold loss is more likely to provoke panic and a full-fledged financial crisis than an equal increase in Amercan liabilities. Hence, the United States should no; be satisfied merely to close its normal deficit. It must take additional steps to make the dollar more attractive as a reserve currency—to increase foreign tolerance of the dollar so as to forestall gold losses when we again cross over into temporary deficit.

The Bank of England and other gold-holding central banks must be persuaded to modify their asset policies—to hold larger dollar balances when their reserves increase. This, I presume, is what Furth meant by "neutralizing" the impact of private capital movements. To this end, the United States might offer foreign governments higher interest rates, by way of special Treasury obligations eligible only for foreign official purchase. It might reassure them with an exchange-rate guarantee, either unilaterally or by way of the IMF or the new OECD. And it might eliminate the 25 per cent gold reserve requirement that now seems to immobilize some 12 billion dollars of the Treasury's gold.

The United States should also have regular recourse to the IMF. Bernstein has observed that the world's largest deficit country is also the largest creditor at the IMF. This anomaly would gradually vanish were other governments to buy the currencies of the surplus countries when drawing on the Fund. It could be ended more rapidly were the United States itself to buy foreign exchange for the IMF. Yet I am a bit worried about the psychological consequences of direct American drawings on the Fund. Until the world comes to regard a drawing as an ordinary occurrence, the United States might attract dangerous attention to its reserve position if it were to make a large purchase from the Fund. Direct American drawings will only be possible after Bernstein's other proposals gain full acceptance from the Fund—after national quotas have been incorporated into national reserves and drawings on the Fund have become more fully automatic.

The measures I have just listed might not support a very large increase of liquid dollar balances. We must consequently ask whether future cash requirements can be met from new gold production or other sources. The world will probably need more cash than gold production can furnish, and we may therefore have to reform international monetary arrangements. If the world has doubts about the dollar, it is because present methods of manufacturing liquidity have placed an incredible strain on our currency. We may not be able to dispel the foreigners' suspicions unless we devise new ways of creating liquidity (as Triffin suggests) or shore up the gold-exchange standard with arrangements like those Bernstein proposes. I hope that future meetings of the Association will provide an opportunity for more discussion of this issue.

JAROSLAV VANEK: I have enjoyed the report of Dr. Bernstein very much. The analysis is well founded on empirical observation and gives proof of a good deal of reflection on the subject. In many respects I agree with Dr. Bernstein's analysis; I will not elaborate at length on these points as they were well stated. Instead I would like to state as briefly as possible the points

where our opinions do not agree perfectly and give my reasons for dissent. I do not find any flaws in the logic of Dr. Bernstein's analysis. Rather, I would like to question some of the assumptions and the emphasis of Dr. Bernstein's discussion of the adequacy of United States monetary reserves.

The important fact brought up by Dr. Bernstein is that the adequacy of reserves in the particular case of the United States cannot be tested in any simple or usual way. Two criteria have to be used: one related to the state of the balance of payments; the other to the holdings of dollars by foreign countries.

The first criterion is one commonly used; namely, the ratio of reserves to imports. As Dr. Bernstein shows, this ratio is satisfactory, although not as favorable as it was some years ago. The justification of such a criterion is, in Dr. Bernstein's opinion, the necessity of preserving a given level of imports, should export proceeds fall. Thus the corresponding demand for international liquidity may be understood as precautionary.

In the minds of at least some international economists, demand for monetary reserves seems to be made dependent on the aggregate value of transactions; that is, it is explained as transactions demand. In this respect, I would agree with Dr. Bernstein in using the precautionary rather than transactions standard. But if we do this, the question arises whether the reserves-import ratio is a satisfactory index of reserve adequacy. In my opinion, it is not. The expected value of possible balance-of-payments deficits and their dispersion (i.e., standard deviation) are two indexes a good deal more important than the ratio of reserves to the value of imports. With any size of trade, with zero mean deficit and zero dispersion, there would be no need whatsoever for monetary reserves.

Strictly speaking, it is only the dispersion criterion that should be considered; the mean expected deficit over any longer period of time should be zero. If the latter index is negative, we are facing a structural disequilibrium, and the only sensible conclusion is that no reserves are adequate. And even if they were sufficient to keep the balance of payments going for some time, it would be better to undertake corrective measures immediately. As for the expected dispersion of balance-of-payments outcomes, there is very little reason to expect that this index would vary in proportion with the value of imports. Statistically, it has not been proportionate in the past. Many other factors besides the value of trade have been influencing this statistic.

The other reason why we have to have gold reserves, as pointed out by Dr. Bernstein, is the large amount of short-term dollar liabilities to foreign countries. At present we do not have even enough to redeem all such commitments in gold. The cardinally important factor here is confidence in our currency. This, in turn, is conditioned by the ratio of our gold holdings to such short-term debt and by the current state of our balance of payments. Dr. Bernstein correctly argues that the latter factor may be the more important of the two.

Two additional remarks, going beyond Dr. Bernstein's analysis, seem to be in place. It is clear that if our balance of payments does not undergo a permanent improvement in the near future, this will worsen our reserve position in all respects. While the import-reserve ratio would drop in proportion

to the deficits, the confidence factor may deteriorate more than in proportion. Actually, where collective psychology rather than quantitative factors are present, an avalanche effect may be expected once a critical state is reached. It should be noted here that short-term rates, in the three to four years ending 1959, were on the average about 2 per cent above ours in the leading financial nations outside the United States. Yet in that time, several billions of short-term dollar funds flowed into the United States. Our comparative interest rate position is not likely to improve in the future, but long-run factors that have caused part of the reversal in our short-term capital balance in 1960 are likely to persist or even be strengthened unless substantial improvement in the over-all balance is reached.

My concluding remark on Dr. Bernstein's paper pertains to his proposal of remedy. Because he has only sketched his proposal rather than providing its full statement, let me raise a few questions, not make a direct critique of the plan. Dr. Bernstein himself states that there is no shortage of liquidity in the world today. Yet he would design an institution that would solve a nonexistent problem, as a precautionary measure for any over-all liquidity shortage that might arise in the future. He expects such a shortage to arise because reserves are unlikely to grow at a satisfactory rate.

On grounds explained earlier in this note, we really cannot say whether there will be a shortage of liquidity in the world even if reserves grow only at the low rate expected by Dr. Bernstein. The acute existing problem of our own reserve position could be assisted by the Reserve Settlement Account only to the extent that our balance-of-payments difficulties are not structural. Many, however, including some of those in this panel, would say that they are structural. If our balance of payments returns to equilibrium in the near future, not only our own but in all likelihood also world reserve problems will be solved.

From Dr. Bernstein's example it would appear that the credit-receiving and credit-furnishing operation of the RSA would be only bilateral. The question arises, unanswered by the speaker: what would happen if the United Kingdom had incurred a deficit with Germany in the second period rather than the United States? Could it use its active balance previously accumulated with the RSA in settling its debt to Germany? Suppose that in this situation the United States balance of payments were in an over-all deficit; yet advance repayment of a four-year loan would be requested by the IMF.

Finally, we should like to ask whether similar loan transactions could not be performed between the countries involved without the intermediary of the RSA. Possible exchange or other risks would have to be covered by the two contracting countries in either case.

The concluding statement of Dr. Bernstein's paper is somewhat unclear and may lead to misinterpretation. In the first place, the function of both the IMF and the RSA would be quite similar and thus supplementary to each other. Both would facilitate settling of deficits of autonomous payments, whether on current or capital account. Second, it would be more fair to say that it would not be the RSA but rather the central banks who would finance conversion of currencies.

ECONOMICS AND NATIONAL SECURITY

THE PROPENSITY TO REDUCE THE NATIONAL DEBT OUT OF DEFENSE SAVINGS

By EMILE BENOIT
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This paper presents some preliminary considerations with regard to one phase of a research program on economic adjustments to disarmament now being conducted with Carnegie Corporation and Ford Foundation financing. It is our hypothesis that the rapid diffusion of nuclear weapons and further technological breakthroughs may render the present system of balanced deterrents increasingly unstable and give rise to a new interest in other types of international security arrangements, involving substantial reductions in national armaments. We are therefore applying input-output, regional, flow-of-funds, and national income analyses to measurement of the potential impacts and are considering also the institutional changes required to facilitate readjustment and growth thereafter.

In this context, the propensity to reduce the national debt out of defense savings is viewed as one factor that might aggravate the stabilization and growth problems in the wake of major arms reduction. We use the term "propensity" in the dictionary sense of "mental disposition" or "tendency" rather than as a strict mathematical function of income or employment. It corresponds to Keynes's "subjective" factor in the propensity to consume; namely, "those psychological characteristics and social practices and institutions which . . . are unlikely to undergo a material change over a short period of time except in abnormal or revolutionary circumstances." (*General Theory*, page 91.)

The evidence for this propensity is both in what people say and in what they do. Consider first what they say. No American politician to my knowledge has ever denied in public the desirability, in principle, of reducing the national debt. Reservations have been entirely with respect to starting, or doing much, during a period of heavy defense expenditures. Most seem to agree that lower defense budgets would provide a favorable opportunity.

Public opinion on this vital point has not been sufficiently examined before, but our program is making a pilot attempt in this field. By cour-

tesy of Professor George Katona and the University of Michigan Survey Research Center, I am able to report here some preliminary findings from a recent national opinion survey by the Center.

To the question, "Supposing our government were to spend less on arms and defense, what would you say should be done with the money saved?" 13 per cent of the respondents and 21 per cent of the college educated spontaneously suggested reduction of the national debt. Moreover, when shown a card suggesting several alternatives, 20 per cent chose debt reduction as the best use of the money, and another 15 per cent chose this as the second best use. In general, debt reduction received about the same amount of support as the proposal to use the money to reduce income taxes. This ran behind the support for fill-in public works and welfare programs, but far ahead of the proposal to increase financial help to other countries. One conclusion emerging from this survey is that, especially among the college trained, a strong minority feels that reducing the national debt is very important, and that substantial savings on defense would provide a favorable chance to accomplish this.

We turn now from words to deeds. Twice—after World War II and after the Korean war—defense expenditures were cut, and this was associated with, or closely followed by, debt reduction. On the first occasion, from 1945 to 1948, annual defense expenditures were reduced by 64.3 billion dollars, and a reduction of 13 billion in the publicly-held debt was achieved in 1947 and 1948. In the second period, from the first half of 1953 to the first half of 1956, the annual rate of defense purchases was cut by around 11 billion dollars, and this was followed by a reduction during 1956 and 1957 of 8.7 billion in the publicly-held debt.

If these experiences serve to reinforce the propensity to reduce the national debt out of defense savings and to suggest that a major disarmament program would provide an ideal opportunity for heavy debt reduction, they could prove seriously misleading.

In 1946-48, the economy was in a quite abnormal state of hyperliquidity, arising from 255 billion dollars of wartime deficit financing, and the chief immediate problem was restraining excessive expenditure. To this inflationary situation was added the fuel of 10.9 billion dollars in *ex ante* tax cuts in 1945 and 1948. Despite all this, the defense cuts and debt reduction brought about a massive decline in industrial production, employment, and national income—which under the circumstances was viewed as normal and desirable but which in the light of our growing world responsibilities may now be reassessed as excessive. And by 1949, we were already in a recession.

In the post-Korean situation, a relatively small defense cut imposed at a time of high-level activity was closely associated with the 1954 re-

cession. It was only after the tax cuts of 1954 had taken hold and rapid expansion in 1955 had raised the revenue base that the surpluses of 1956 and 1957 became possible. These, in turn, were accompanied by a new slowdown in industrial growth rates leading into the large passive deficits of the 1958 recession. Efforts to achieve a surplus for debt reduction have again led to a disappointing growth rate and an unpromising outlook in 1960.

One reason why the propensity to reduce the debt out of defense savings may have such disappointing results is that it is generally associated with attempts to block or delay compensatory fiscal policies (supplementary nondefense expenditures or tax cuts) required to sustain income and employment, on the plea that it would be more prudent to wait and see how large a budget surplus actually materializes before starting to dissipate it.

Such delays may, however, be dangerous for economic stability. A deflationary impact is likely to be felt from a reduction in defense orders, even before there is any significant decline in actual defense expenditure. Thus in 1953, between the second and fourth quarters there was a decline of 2.8 billion dollars in defense obligations for hard goods, with an associated drop of 9.7 billions in new orders received by the durable goods industries and a 4.3 per cent decline in industrial production. Yet actual defense expenditures in the fourth quarter were only about half a billion dollars lower than in the second quarter. Again in 1957, between the first and third quarters there was a 1.5 billion dollar decline in defense obligations for hard goods, with 5.5 billion decline in new orders received by the durable goods industries. By the last quarter of the year, industrial production had dropped off by 5.9 per cent despite the fact that the dollar value of national defense expenditures had been rising and was still running slightly ahead of the first quarter. Although the whole blame for the industrial decline in these cases cannot be laid to the cuts in defense orders, they certainly appear to have precipitated the declines.

These experiences suggest that attempts to achieve budget surpluses for debt reduction by means of defense cuts may under all but exceptionally inflationary circumstances prove largely self-frustrating. A reduction in defense orders reduces economic activity, national income, and the revenue base. The ensuing decline in actual government payments would reinforce this trend even if equally and promptly offset by tax cuts, owing to the operation of the balanced budget multiplier. In the future, therefore, substantial surpluses may be attainable, if at all, only on a rapidly rising trend of income and revenues.

If budget surpluses are, nevertheless, obtained as a postlude to arms cuts, the cost in lost output, employment, and income may be high. The

deflationary impact of budget surpluses is generally agreed upon by economists. A preliminary application to this problem of Professor Daniel Suits's national income forecasting model suggests severe declines in GNP if any substantial part of defense savings through a large disarmament program should be applied to a retained surplus.

Assuming, however, that a surplus is attained, the use of it to retire the national debt could have some expansionary effect, depending in part on who holds the debt. Retirement by the Treasury of government obligations held by the Federal Reserve System has a wholly neutral effect. Retirement of federal securities held by individuals, businesses, and state and local governments, on the other hand, may have some stimulative effect. By substituting bank deposits for securities, it at least adds to private liquidity. However, short-term government securities and bonds approaching maturity, which the government would retire, are already highly liquid instruments, and it is hard to imagine that the savings and investment decisions of the holders would be greatly influenced by the slight increase in liquidity involved. It might, nevertheless, lead to their holding somewhat larger cash balances than formerly, and thus would increase bank liquidity and ability to make loans.

The net effect would be about the same as if the government obligations retired were those held by the private banks: a rise in bank reserves with the Federal Reserve System and a greater freedom to make loans. How effective this would be under normal circumstances is questionable. The effect might be appreciable if the banks were loaned up, if there were plenty of people wanting to borrow, and if tight money policies were effectively restraining activity. Some question whether this ever actually occurs. They point out that funds are increasingly available from outside the direct control of the banking system and that under pressure credit can be more effectively utilized, with a rise in the velocity of circulation.

In any case, if credit ease is desired, there are simpler ways of getting it than by debt retirement. Purchases of government obligations by the Federal Reserve System in open market operations will be even more effective. Reserve requirements can also be lowered—even if there is hesitation to lower the rediscount rate for balance-of-payments reasons!

Thus it appears that the combined effects of having a surplus and retiring debt with it will under most conditions be markedly deflationary. Debt reduction may improve private liquidity and facilitate private borrowing, but running a surplus curtails markets and thereby usually weakens the incentive to borrow. One can, of course, imagine circumstances (such as a disarmament program coming at the end of a new massive deficit-financed armaments boom) where budget surpluses and debt reduction might have a positive role in simultaneously repressing

total expenditure and consumption and shifting liquidity away from the federal government towards state and local governments and private investors. Such conditions seem highly unlikely at the moment, and under the conditions such as we have had since 1951—and which seem likely to prevail in the future—the persistant pursuit of debt reduction may prove not only inappropriate but positively dangerous.

In this connection, I would call attention to a seminal but strangely neglected article by Paul McCracken, "The Debt Problem and Economic Growth" (*Michigan Business Review*, November, 1956), which suggests that certain structural characteristics of the way our savings are channeled into investment may make a long-term rise in public debt indispensable for economic growth. This could help explain why our recent attempts to reduce debt have proved so disappointing and so frustrating. In a period of intensified economic rivalry which might follow a reduction in military tensions, this point could assume increasing importance, with our debt reduction propensity becoming a major source of national weakness.

In recent technical literature, attempts have been made to revive, on a more sophisticated basis, concerns already abandoned by most economists about the "burden" of an internally held debt. This burden, in its new form, assumes a somewhat metaphysical character, not incompatible with everyone being absolutely better off than if the debt had not been acquired. It is devoutly to be hoped that in facing the very real dangers ahead, we shall not have our vision clouded by chimeras of this kind. Economists should be especially careful, I feel, not to permit their abstruse professional speculations to be misinterpreted as providing support for popular economic fallacies. And of all such fallacies, the belief in the burdens and perils of the public debt seems to me the most widespread and the most dangerous.

THE IMPORTANCE OF INDIVIDUAL INDUSTRIES FOR DEFENSE PLANNING

By Donald V. T. Bear, *Stanford University*
and Paul G. Clark, *Williams College*

Economic Recovery and Defense Planning

The object of our analysis[1] is to examine, as a guide to peacetime defense preparations, plausible supplies and demands in the U.S. economy after a nuclear war. In view of the repugnance with which all of us approach this subject, let us make clear certain presumptions about its relevance for defense policy.

To begin with, although deterrence of general war is the primary goal of defense preparations, deterrence may fail in a variety of circumstances. To meet this residual risk of war, some insurance measures to ameliorate its consequences if it occurs are a sensible secondary goal. Moreover, many patterns of military action are conceivable which permit us to envisage a severely damaged postwar society, but with enough resources surviving to permit economic recovery over an extended period of time. Finally, individual industries might differ significantly in their contribution to economic recovery.

The measure of importance which we consider is based on a comparison between estimated postwar supplies and estimated postwar demands for the products of some twenty individual manufacturing industries. Prospective supplies derive from plausible assumptions about the productive resources surviving enemy attack, as affected by the industrial specialization of different-sized target cities. Prospective demands derive from plausible assumptions about postwar consumption, investment, and government expenditures, as constrained by the reduced real national income in relation to surviving population. In essence, we explore the implications of using the ratios of prospective demand to prospective supply as a first approximation to postwar values.

We should stress that our concept of postwar value refers to a "recuperation period" beginning six months to a year after a war and extending for perhaps a decade; we distinguish this period from the immediate "reorganization period." It should also be noted that our full calculations are carried through for only a single combination of wartime destruction and postwar demands, which we call our "standard case." We

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[1] A fuller version is given in P-2093, The RAND Corp., Sept. 15, 1960 (Unclassified). Data used in the study are given in P-2124, The RAND Corp., Oct. 18, 1960 (Unclassified).

believe that this is a representative and meaningful case, and at several
points have considered the sensitivity of our results to a wider range of
assumptions, but fuller examination of other cases would be desirable.

Prospective Postwar Supplies

The first stage in our supply analysis involved defining a list of po-
tential economic targets. Our list includes all cities with a 1954 value-
added in manufacturing (MVA) of at least 40 million dollars or a 1950
population of at least 50,000. This dual criterion defines a list of 300-odd
cities, which together produced a little more than three-fourths of total
MVA in 1954 and contained a little less than half the population.

Then in each of the 132 largest cities aggregate MVA was broken
down into the 20 individual industries at the two-digit level in the Stand-
ard Industrial Classification. If we now rank U.S. cities by aggregate
MVA, striking differences emerge in the concentration of individual in-
dustries in the larger cities. The top 30 cities contain approximately
three-fifths of instruments production but only about one-fifth of textile
production, with the remaining industries (other than widely dispersed
lumber) ranged in between. Thus if an enemy successfully attacked the
largest targets, substantial differences in the survival of individual in-
dustries could reasonably be expected.

Moreover, the differences in concentration do not change much as the
number of targets rises from 30 to 130. Half of the industries change
concentration rank by two or less over this range, and only one (to-
bacco) changes rank by more than four. The point is that the largest
cities contain such a subtantial fraction of total MVA that their patterns
of concentration tend to be preserved as additional smaller cities are
added to the list. Accordingly, in defining our standard case we have con-
sidered an 80-city attack to be representative of attacks over the entire
range from 30 to 130 cities.

There is still a question as to alternative criteria which an enemy
might use to select city targets. We have therefore considered several
variant 80-city attacks. One selected cities ranked by value-added in
the durable-goods industries. Two other variants, representing bottle-
neck attacks, selected cities ranked by value-added in the metals and
petroleum industries, respectively. These alternative criteria proved to
have surprisingly little effect, except for the single industry out of twenty
selected as a bottleneck objective. The point is that size differences
among cities are so great that altering the selection criterion does not
markedly alter the target list; 68 of the 80 cities in our standard case
were also included in the durable goods variant, for instance.

For our standard case, the potential supply indexes on a prewar
base for individual industries (see Table 1, column 1) range from 28

TABLE 1

COMPARISONS OF PROSPECTIVE POSTWAR SUPPLIES AND DEMANDS
FOR INDIVIDUAL MANUFACTURING INDUSTRIES

	Supply* (Prewar = 100)	Demand (Prewar = 100)	Importance Ratio (D ÷ S)
Instruments..........................	29	56	1.93
Printing..............................	31	49	1.58
Nonelectric machinery.................	39	61	1.56
Fabricated metal products.............	37	57	1.54
Electric machinery....................	38	56	1.47
Motor vehicles.......................	28	41	1.46
Nonferrous metals†	39	53	1.36
Ferrous metals.......................	39	52	1.33
Other transport equipment............	28	35	1.25
Miscellaneous manufactured products....	31	40	1.18
Apparel..............................	32	34	1.06
Food................................	49	51	1.04
Rubber†	39	40	1.03
Stone, clay, glass†	63	61	.97
Petroleum...........................	51	49	.96
Chemicals............................	53	51	.96
Furniture............................	51	46	.90
Paper................................	63	49	.78
Lumber..............................	84	61	.73
Tobacco..............................	60	40	.67
Radio-television equipment............	38	25	.66
Leather..............................	61	40	.66
Textiles..............................	70	35	.50

* Supply index for SIC 36 attributed to both electric machinery and radio-television equipment; for SIC 33 to both nonferrous metals and ferrous metals; for SIC 37 to both motor vehicles and other transport equipment.

† Use of the existing mobilization stock pile over three years might raise the supply index for nonferrous metals to about 47, for rubber to about 48 (if crude rubber were first converted to finished products of equal value), and for stone, clay, glass to about 68.

per cent to 84 per cent. These indexes are simply the percentages of peacetime value-added produced outside the 80 cities assumed to be destroyed; we have not adjusted either for continuing disorganization of production in undamaged cities or for more intensive emergency utilization of surviving resources.

Finally, let us note one aggregative implication of our 80-city attack. Although it would deal a crippling blow to the economy, about 44 per cent of aggregate MVA would survive. Since nonmanufacturing activity is more widely dispersed, it is reasonable to suppose that surviving resources could produce something like half of peacetime GNP if effectively utilized.

Prospective Postwar Demands

Two general premises underlie our demand analysis. One is that potential GNP after reorganization would be about 50 per cent of prewar, as just suggested. The other is that the proportion of GNP devoted to fixed investment and government would increase, primarily as a result

of political mechanisms favoring rapid reconstruction. This in turn is related to our guess that in our standard case something like 60 per cent of the population would survive. We have calculated that restricting per capita disposable income to roughly two-thirds of prewar would then permit the share of GNP devoted to investment and government to be raised from 33 per cent to 43 per cent.

We have also considered cursorily the sensitivity of our demand analysis to other assumptions. We judge that it is not sensitive to varying levels of attack, within the range of 30 to 130 cities, since both economic destruction and population casualties would be similarly affected. It surely is sensitive, however, to differences in population survival resulting from substantial differences in fallout patterns or in prewar civil defense preparations.

The first stage in our demand analysis was to assume a plausible pattern of final demands upon the limited postwar GNP. For consumption, we employed income elasticities for some thirty kinds of consumer goods based on the experience of the thirties. For other components of GNP, we adopted such assumptions as the following: that government expenditures would shift from defense to reconstruction and medical services, that imports would decline with the fall in domestic production while exports would virtually cease, and that agricultural stock piles would be used up over a three-year period. The remainder of postwar GNP was assumed available for fixed investment.

The next stage was to translate the final demands into a set of total demands on individual manufacturing industries. We used for this purpose an adjusted version of the 1947 interindustry matrix, correcting at least roughly for discrepancies in updating it to 1956.

The results have been stated as demand indexes on a prewar base (see Table 1, column 2), and range from 25 per cent to 61 per cent.

Implications of Demand-Supply Ratios

Our estimates of postwar demands and supplies, it should be clear, are estimates of points on functions rather than the supply and demand functions themselves. In reality, an entire production possibility surface would survive an attack, and we would expect many conversions and substitutions in production. We have simply estimated that point on the surface which would represent the supply vector if relative prices were unchanged. Similarly, given postwar income and the public "taste" for reconstruction, we would expect many substitutions on the demand side. Again, we have simply estimated a point on the community preference surface (corresponding to the budget implied by the supply analysis) with slopes reflecting prewar relative prices.

We now wish to employ this limited information to speculate about

postwar relative values, as they might be reflected in a dynamically stable price system adjusting to a new equilibrium. Lacking the necessary substitution information, we have simply calculated the ratio of prospective demand to prospective supply for each industry. We look upon this "importance ratio" as the best available, though crude, index of postwar change in relative value.

The range of the importance ratios (see Table 1, column 3) is quite wide. The ratio for instruments at the top of the list is nearly four times that of textiles at the bottom; moreover, the ratios for the top six industries are all at least double those of the bottom five. It is also notable that the range of importance ratios is relatively greater than the range of either supply or demand indexes considered separately.

The particular industries that appear near the top of the list deserve comment. The first nine, with the exception of printing, are all metal-working durable goods industries. These industries have lower than average survival rates; they also seem likely to face stronger than average demands under postwar circumstances.

We suggest that a ranking of industries with measures like our importance ratios could be a useful guide to civil defense policies of an economic character. For example, more than two-thirds of the 8 billion dollar mobilization stock pile consists of various nonferrous metals, and more than one-sixth is rubber. In light of the importance ratios it is questionable that this is an appropriate composition. Another possible application is to guide research in the best mix of measures to sustain postwar supplies from especially important industries—such as the metal-working durable goods industries suggested by our analysis.

STRATEGY FOR ACTIVE DEFENSE

By THORNTON READ
Bell Telephone Laboratories, Inc.

The term "strategy" is used here to denote the broad, ill-defined area between weapon systems engineering on the one hand and military policy planning on the other. Our purpose is to illustrate the application of scientific methods in this area by discussing the special case of an active defense against ballistic missiles. By an active defense we mean one that attempts to intercept an attacking missile before the warhead is detonated (as distinct from passive defense which provides protection against the explosion).

During and after the second World War the scientific methods which are standard in military systems engineering have been applied more and more to questions concerning the use, choice, and evaluation of weapons; that is, to the questions discussed here. This activity has included what is called operations analysis, systems analysis, war gaming, and game theory. What follows here will touch on these disciplines as they apply to problems of active defense against ballistic missiles.

Scientific methods are, of course, more applicable to problems lying closer to the engineering side of the spectrum and it is these on which we shall concentrate. Indeed, the effort to extend scientific methods (in the strict sense of the term) too far in the direction of policy can be dangerous. It may lead to misleading oversimplifications, the forcing of subject matter into alien forms, or the overemphasis of peripheral factors which happen to be mathematically tractable.

Although this paper will be more concerned with problems closer to engineering than to national policy, we shall have little to say about hardware as such; that is, about the details of the missiles, radars, and so forth that would make up a defensive weapon system. Rather we shall discuss such questions as: how should defensive weapons be deployed before an attack to minimize the damage that could be done to the defended targets; how should defensive missiles be committed during an attack; what is the effect on both offense and defense of the fact that the actual performance of weapons in combat is highly uncertain; how should the characteristics of weapons, insofar as they can be known or estimated, be translated into terms that would help a nontechnical executive relate the system to the objectives of national policy and decide whether to produce and deploy it in quantity? These questions together with many other related ones cover a wide range. We cannot

hope to deal with the subject in any completeness here. What will be attempted rather is (1) to give a broad-brush over-all view of the subject and (2) to go into just enough depth in a few places to give some flavor of the concrete results.

First consider the question of method, which is pertinent to strategic problems whatever the subject matter. Roughly there are two ways of proceeding. One is to set up a simple model that can be understood conceptually and treated analytically. This has the advantage of being understandable in the sense that the relation between assumptions and conclusions can be appreciated. The disadvantage is that it is always oversimplified and lacking in realistic detail. The other procedure is to develop a big detailed model and use high-speed computing machines to handle the detail. Perhaps the best examples of this are the various air battle analysis models in which the inputs to the computer include the flight and refueling plans of the bomber force, the assignment of bombers to targets, the detection of bombers by radars, and their engagement by interceptor aircraft and local antiaircraft missile batteries. The big model includes a wealth of detail but it provides relatively little conceptual insight. A large number of highly conjectural assumptions have to be made. These go into the computer in the form of equations. The relation between assumptions and the output of the machine is not well understood and the results usually become detached from the assumptions on which they were based and apart from which they are meaningless. This detachment is facilitated by the way in which the results are usually presented to the decision-makers who look to them for guidance. Formal briefings are ideal for displaying quantitative results in the form of charts and graphs. They are less than ideal for examining the conceptual subtleties of the assumptions which made it possible to reduce a complex problem to neat results.

The more sophisticated defenders of the big model would say that it is not intended to give precise predictions, that its value is heuristic and educational. It is the best substitute for actual combat experience; it permits a commander to test his intuitions against a specific set of conditions, however hypothetical. If the model has been developed in close contact with the commander and his staff, then it should be at least an accurate representation of the command's view of the world. Thus it helps the command discover previously unrecognized consequences of perhaps too hastily accepted assumptions. Surely a commander who has an internally consistent view of his world will be in a better position to cope with external uncertainties. In short, the main value of the big model is that it gives understanding and insight, not accurate detailed predictions. The details are spurious.

However, if the value of a model is that it gives understanding, then

there is much to be said for a simple model which gives a maximum of understanding with a minimum of the spurious detail which obscures the essential insights and fosters a deceptive sense of realism. Admittedly the simple model oversimplifies the real world, but all human reasoning operates with concepts that are oversimplifications. One can ask only that the oversimplifications be useful, that they highlight some essential feature of the problem, rather than just give an elegant treatment of the most tractable features. The danger of trying to be too scientific in the treatment of policy is that as we go from engineering toward policy the essential and the mathematically tractable tend to overlap less and less.

For purposes of this discussion quite a simple model will suffice both to illustrate a few concrete results and to indicate the scope of the analysis. The model is sufficiently general to apply to a wide range of specific weapons systems. Perhaps the best way to introduce the model is by defining the terms that will be used in the discussion.

A "target" is what we are defending. It could be a city or a bomber or missile base, for example. An offensive missile will be called an "attacker." Thus the offense shoots attackers at targets. We shall assume that if a single attacker penetrates the defense, it will in some meaningful sense "kill" the target so that the full value of the target will be lost. Although this is only an approximation, it is (regrettably) not a bad one for modern weapons. The defense tries to "intercept" the attackers by firing defensive missiles at them. Unless otherwise specified, we shall consider the case of a local defense, where each defensive installation covers only one target. Besides missiles, the defensive weapon system includes the radars and computers required to guide them to a successful intercept. We need not describe the defensive hardware in any detail. For the purposes of our analysis it is sufficient, and in fact preferable, to give a general functional description which brings out the defensive limitations on which a well-designed offense would seek to capitalize. Let us therefore take the following general description: The defense "fires ammunition" (defensive missiles in this case) at "attackers." Each of these quoted words indicates one of the limitations of the defensive system. At each defended target the defense is limited by: (1) the local stock pile of ammunition, or defensive missiles; (2) the number of defensive missiles that it can fire at a group of attackers whose "times on target" are as nearly simultaneous as offensive missile technology allows; and (3) the number of attackers that can be simultaneously engaged.

Both (2) and (3) are determined by the local guidance equipment; that is, by the number of tracking radars and the capacity of the guidance computer.

Thus without going into the details of the hardware we have set up a model defined by the above three parameters, to which one more must be added; namely, the probability that a single defensive missile will intercept the attacker which it engages.

In summary, the offense shoots attackers at a target. The defense shoots defensive missiles at the attackers. Unless *all* of the attackers are intercepted the target will be killed. This describes the model. Now let us look at the implications in terms of defensive and offensive tactics.

The interaction of offense and defense can be visualized as a three-move game with the following moves: (1) The defense deploys equipment to targets. This must be planned months in advance; once installed, defensive hardware cannot be quickly shifted from one target to another. The disposition of defensive hardware is known to the offense. (2) The offense assigns attackers to targets. This can be done shortly before attack; the assignment is not known to the defense at the time the defense is deployed. (3) The defense assigns defensive missiles to attackers as they come within the field of defensive fire. What this assignment will be cannot, of course, be known to the offense when he fires.

Against any one target the offense may try to make all of the attackers arrive simultaneously so as to saturate the defensive tracking capacity, or he may prolong the attack so that only one attacker is engaged at a time. In the former case, move (3) above is trivial. The defense fires as many missiles as he can in the short time available and assigns missiles to attackers as uniformly as possible. (No prizes are given for scoring several intercepts on the same attacker while allowing another to penetrate and kill the target.) Thus in the case of a simultaneous attack the first two moves define the strategic problem. In this two-move game the offense has the last move and makes it knowing the defense's move. The offense optimizes his attack against the existing and known defense. The defensive deployment, by contrast, is optimized against not a specific attack but against the kind of attack an intelligent offense would launch when he sees what the defense has done. This is what makes the defensive deployment problem intriguing. There is not space here to present the solution, although it can be fairly well expressed in non-mathematical terms.

Here it may be asked whether the defense is not conceding too much in giving the offense credit for perfect rationality in designing his attack plan. There are several reasons for such an assumption. For one thing, it makes the problem determinate. There are a limited number of ways of being rational; so one can prepare against them. But the number of ways of being foolish is virtually infinite. If we have no convincing rea-

son for expecting some particular irrationality, it is better to say that we will defend ourselves against the worst that the enemy could do. But this is just what he would do if he were perfectly rational and were trying to do what we are trying to prevent him from doing. (Clearly such an assumption cannot safely be extended beyond the competitive aspects of military problems where the interests of the two sides tend to be mutually exclusive.) If the defense assumes the offense to be intelligent (in the above sense) and the offense turns out to be stupid, the defense will suffer less damage than he expected. But if we assume the enemy will be stupid and he turns out to be intelligent or to be stupid in a different way, then the damage may be much greater than expected. The assumption is therefore conservative.

Let us now go on to consider the case where the offense prolongs the attack so that the defense engages one attacker at a time but where strategic requirements do not permit the offense to wait to see whether one shot has killed the target before firing the next shot. Here we have what is called a pure game. Once the defense is deployed, the offense fires a number of attackers at a target. When he fires he does not know what firing doctrine (or schedule of assigning defensive missiles to attackers) the defense will use. Likewise, each local defense must decide on a firing doctrine without knowing how many attackers are coming. For example, to take the simplest case, suppose the defense had only two missiles. He could either fire both of them at the first attacker to arrive or fire one at the first and save one in case another attacker should follow. For an attack on a set of defended targets, we can make a list of attack strategies (defined by the number of attackers per target) and a list of defensive strategies, or firing doctrines, and calculate the expected number of targets killed for each pair of strategies. Thus the problem itself generates the kind of matrices which are the subject matter of game theory.

In the case of the simultaneous attack, the defense fires knowing the attack size and hence is able to make the best use of his defensive missiles. In the prolonged attack the defense does not have this advantage, but he is not limited by his radar tracking and guidance capacity and so is able to use his entire stock pile of defensive missiles. The ratio of the total missile stock pile to the number of missiles that can be fired against a simultaneous attack should be such as to give the defense equal protection against the two types of attack: simultaneous and prolonged. This tells the defense how to divide his resources between missiles on the one hand and guidance and tracking equipment on the other.

To derive a few more specific results, let us consider the simultaneous attack more closely. Suppose, for simplicity, that we have a number of equally valuable and equally well-defended targets, such as air or

missile bases. How would the offense attack them. He has the choice of lightly attacking a large number of the targets or of concentrating his force in heavy attacks on a smaller number. The best attack strategy depends on the nature of the defense—a question which is widely misunderstood. The common view is that an active defense is like a sieve. It stops a certain fraction of the attackers and allows the rest to penetrate. Semitechnical discussions of defense tend to revolve around the question of what fraction would penetrate. Actually, however, a local defense has much more the character of a brittle barrier which stops attacks below a certain critical size and allows larger attacks to penetrate. In the limiting case of perfect defense missiles (having 100 per cent intercept probability), the defense would be perfectly brittle. For example, if the defense could fire ten defensive missiles, it could stop an attack by ten or less attackers, while an attack by eleven or more attackers would be sure to kill the target. Thus the probability of killing the target would jump abruptly from zero to 100 per cent as the attack size increased. In the realistic case of imperfect missiles (with an individual intercept probability less than 100 per cent), the probability of killing the target rises less abruptly, but the assumption of a brittle barrier is still a good approximation—certainly much better than the sieve approximation.

In all realistic cases the optimum attack is uniquely determined by the local defense and has a high probability of killing the target. The optimum attack represents a balance between two conflicting tendencies. Too large an attack makes it virtually certain that the attacked targets will be killed, but it tends to waste attackers in overkilling some targets. These attackers could more profitably be used to increase the number of targets attacked. On the other hand, committing too few attackers to to an attacked target runs the risk that the attack will fail to penetrate and so the whole attack on that target will be wasted. Because wasting a few attackers in overkilling is less serious than wasting the whole attack, the optimum attack leans toward the former. Thus an optimum attack is almost sure of killing the target.

This result suggests a view of active defense at variance with the common, naïve one according to which the purpose of defense is to save targets which come under attack. Actually the purpose of defense is to put a "price of admission" on a target. If the offense wishes to knock out a target, he can do so by mounting the required attack against it; that is, by paying the price of admission (usually just called price). But if he knocks out some targets, he will not be able with a limited total attack force to attack other targets. The defense of target A will not save target A if it is attacked. But it may have saved target B because

the offense had not enough attackers to overcome the local defenses at both targets.

If this point were well understood, many futile arguments over defense could be avoided. It is often said, for example, that active defense is useless because some attackers will always get through. Any defense can be penetrated. The answer is that, of course, the offense can penetrate the defense anywhere by making a sufficient effort. But the fact that he can penetrate anywhere does not mean that he can penetrate everywhere. The purpose of a nationwide deployment of defense is to force any offense to use up so much of his force in penetrating some defenses that he cannot attack all of the targets.

Again it is asked how we can know the defense will work under the untested conditions of an actual attack. But suppose the defense fails utterly. What happens? All of the targets attacked are destroyed. But as we have seen, an optimum attack assures that virtually all targets attacked will be destroyed in any case. The purpose of the defense is to prevent some targets from being attacked and it will have accomplished this if the offense thinks the defense will work and attacks accordingly. The primary question therefore is not, how do we know the defense will work, but how does the opposing offense know it will not work? He is able to capitalize on its failure only if he knows it will fail and attacks accordingly. A defense thus shifts the burden of proof on the offense.

This point should not be overlooked—especially when the purpose of strategic weapons is so largely deterrence.

This brief survey has tried to indicate the scope and method of the study carried out at Bell Telephone Laboratories and to convey some flavor of the results. Both the general insights and some of the specific results have been useful in planning weapon systems, in formulating possible deployments and tactics, and in evaluating a system as a component in an over-all military program.

THE CRUDE ANALYSIS OF STRATEGIC CHOICES[1]

By Daniel Ellsberg

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Many of the significant implications for U.S. military objectives of specific military choices—such as the introduction of a new weapons systems, a change in basing or deployment, new operational procedures or protective measures—depend upon their impact on the limited set of U.S. and Soviet "decision elements" shown in the following format:

U.S.	Soviet Union	
	Wait	Strike (p)
Wait........................	u_{11}, v_{11}	u_{12}, v_{12}
Strike (q).................	u_{21}, v_{21}	—

We shall not try to define a "game" corresponding to this schema, though the format might suggest that interpretation; rather, it depicts some major, interrelated elements in two concurrent U.S. and Soviet decision problems: whether or not to launch an all-out nuclear attack upon the opponent. "Strike" denotes such an attack; the "Wait" strategy may be interpreted either as a "representative" or as a "best" alternative to Strike. The inclusion of a U.S. Strike strategy does not imply active consideration of such an alternative at any given moment; as a possibility, it is relevant particularly to Soviet calculations, for reasons indicated below.

The u's and v's are, respectively, U.S. and Soviet "von Neumann-Morgenstern utilities"[2] for certain (highly aggregated) consequences of their actions; p and q are, respectively, the U.S. and Soviet subjective probabilities (expectations, estimates of likelihood) that a choice of Wait will encounter an opponent's choice of Strike during a certain time period. Though all of these are subjective variables, they clearly depend upon estimates of objective outcomes under specified circumstances, based upon some form of explicit or tacit "systems analysis."

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[1] This paper is considerably condensed from P-2183, "The Crude Analysis of Strategic Choices" (RAND Corp., Santa Monica, Calif.); P-2183 develops the present approach further and applies it to a number of proposals and propositions in current debate.

[2] I.e., they indicate not merely order of preference among these outcomes but the decision-maker's preferences among "gambles": strategies which offer a set of possible outcomes with given subjective probabilities. It is assumed here that utility numbers can be assigned to outcomes so that the decision-maker's actual choices among strategies can be described as maximizing the "mathematical expectation of utility," the average of these utilities weighted by their respective subjective probabilities.

For purposes of this discussion, the v's may be regarded as U.S. estimates of Soviet utilities—estimates which are not held with perfect confidence.

Symbols u_{21} and v_{21} are thus, respectively, U.S. and Soviet utility pay-offs for "strike first" outcomes: the consequences of a surprise nuclear attack upon the opponent. Symbols u_{12} and v_{12} are "strike second" pay-offs, reflecting the consequences of being struck first by the opponent. Symbols u_{11} and v_{11} are "no all-out war" pay-offs, corresponding to situations in which neither opponent chooses Strike.[3]

The precise effects of a change in military "posture," hardware, policy, or plans upon these eight variables (including p and q) are, of course, hard to determine, uncertain, and subject to controversy; nevertheless, rough estimates are often made, and these are, in fact, the basis for most policy recommendations as to choices among military alternatives. Indeed, many such recommendations reflect estimates of effects only upon some subset (e.g., one) of these eight factors. The above schema has the advantage of directing attention at least to these eight, gross consequences of a military change. A typical, major military innovation will affect all of these variables, and in what may be opposing directions for a given or for different military objectives. Within a given set of strategic policy alternatives, "conflicts" may be inescapable; an improvement in terms of one dimension of choice (one military subgoal) may be unavoidably associated with losses with respect to another. Analyses which ignore several of these dimensions are thus likely to be inadequate. Isolated suboptimizing processes which overlook conflicts and "spill-over effects" among related subgoals may end by lowering over-all military security rather than raising it.

While the above highly simplified and abstract schema can by no means be regarded as an adequate model for the comparison of U.S. military alternatives, it may represent a minimum framework which is an advance over that implicit in much current discussion. Assuming that it is possible to estimate the effects of a military innovation (e.g., an airborne alert, the introduction of IRBM's in Europe, a fallout shelter program) upon the factors in this schema,[4] the question arises: what effects, or complexes of effects, are "good"? For practical purposes the over-all goal of enhancing military security—reducing the likelihood of major losses from the threat or use of enemy military force—must be broken down into military subgoals, a list of specific strategic objectives. Some

[3] The utility subscripts have been chosen to show corresponding elements in the concurrent, related but separate U.S. and Soviet decision problems; if a "game" formulation were being followed, the subscripts for the v's would be transposed.

[4] However these estimates are derived and whether or not they are "reliable," the schema can be helpful in deriving their policy implications, in order to test the "consistency" of given policy recommendations with corresponding estimates and objectives.

of these correspond directly to elements in our schema. Thus it is a major U.S. objective to lower p: roughly, to "improve the reliability of deterrence." Likewise, there is the goal of raising u_{12}: improving the strike-second outcome if deterrence should fail. Possible conflicts between these two subgoals are well known. However, by guaranteeing retaliation (lowering v_{21}) it may be possible to lower p greatly, more than compensating for the lower u_{12} which is associated with the low v_{21}. But what is the effect upon p of improving u_{12}, by planning counterforce tactics, or introducing civil defense? To answer this sort of question we must look at the impact not only upon u_{12} but upon all the elements in this framework, for p depends upon the whole configuration of factors in rather a complex way.

To the extent that a Soviet Strike represents a deliberate decision, it must reflect the fact that in Soviet calculations of pay-offs and likelihoods at some moment Strike appeared preferable to its best alternative. The goal of the U.S. "deterrence" policy is to ensure that this never arises: that at all times Strike appears inferior in Soviet calculations to some alternative ("Wait"). In our schema this condition appears equivalently as:

(1) $V(\text{Wait}) > V(\text{Strike})$, where V is the Soviet utility function; or
(2) $(1 - q)v_{11} + q \cdot v_{12} - v_{21} > 0$; or
(3) $(v_{11} - v_{21}) - q(v_{11} - v_{12}) > 0$.

Even though U.S. estimates may indicate that this condition holds at a given moment, p may not be 0; some U.S. uncertainty ($p > 0$) may remain, reflecting: (a) the possibility that U.S. estimates are critically mistaken; (b) the possibility that factors affecting Soviet calculations may change critically within the relevant period; (c) the possibility that Soviet behavior may be non-calculated, impulsive, or erratic, imperfectly co-ordinated, or subject to "unauthorized actions" by subordinates.

Each of these likelihoods is likely to be smaller, the larger the interval, $V(\text{Wait}) - V(\text{Strike})$.[5] Other things being equal, the "worse" Strike appears relative to its best alternative, then the more likely that the Soviets are "deterred," the more likely that they will stay deterred as pay-offs undergo exogenous shifts, and the more care that Soviet decision-makers will take to avoid accidents, false alarms, hasty decisions, unauthorized actions, or unco-ordinated, unmonitored policies. The size of this interval, then, provides a subcriterion among military choices on the path towards lower p. It is, in effect, an index of the sensitivity of the Soviet decision to "counter-deterrent" shifts in pay-offs (if q is given) such as: (a) a drop in the "no all-out war" outcome v_{11} (due to Soviet losses or

[5] A unit interval having been established by the arbitrary assignment, say, of values 0 and 100 to two specified outcomes.

expectation of losses in a limited war, shifts in prestige, influence or alliances, cold war failures, domestic setbacks or uprisings, political rivalries with third parties); (b) a drop in the Soviet "strike second" outcome v_{12} (due to increased U.S. force size or ability to exploit weaknesses in Soviet warning systems or defenses, or prospect of U.S. "annihilation tactics" in a U.S. first strike); a rise in the Soviet "strike first" outcome v_{21} (a reduction in U.S. "strike second" or retaliatory capability, due to changes either in U.S. or in Soviet posture, procedures, tactics). The larger the interval, $V(\text{Wait}) - V(\text{Strike})$, the larger (in utility terms) the pay-off disturbances required to make Strike appear preferable to Wait. This might be regarded as one index of the reliability of deterrence.

Another important index of this reliability is the sensitivity of the Soviet decision to shifts in q, the Soviet expectation of a U.S. first strike.

To understand why q is relevant at all to the Soviet choice, let us recall the earlier condition of deterrence:

$$V(\text{Wait}) - V(\text{Strike}) = (v_{11} - v_{21}) - q(v_{11} - v_{12}) > 0.$$

Since typically $v_{11} > v_{12}$, it follows that a necessary condition for deterrence is:

$$(v_{11} - v_{21}) > 0, \text{ or } v_{11} > v_{21}.$$

It cannot be taken for granted that this condition will hold; it does not follow automatically from the existence on both sides of nuclear weapons.[6] But in any case, this condition is not sufficient. Perhaps the most significant aspect of the current strategic balance is that, under typical conditions of technology and posture:[7]

$$v_{21} > v_{12}.$$

It follows that deterrence can fail $[(v_{11} - v_{21}) - q(v_{11} - v_{12}) < 0]$ even though $(v_{11} - v_{21})$ is positive and large: if q, the Soviet expectation of a U.S. Strike, is sufficiently great.

An important question is: How high would q have to be to make Strike appear preferable to the Soviets? A threshold value $\bar{q}$, below which the Soviets would be deterred and above which they would prefer Strike, is given by:

$$(v_{11} - v_{21}) - \bar{q}(v_{11} - v_{12}) = 0, \text{ or } \bar{q} = \frac{v_{11} - v_{21}}{v_{11} - v_{12}}.$$

[6] As Albert Wohlstetter has pointed out, U.S. retaliatory power could be so vulnerable to a Soviet "no warning" attack as to promise less destruction than the Russians have suffered historically, whereas the "no war" outcome could, under abnormal conditions, appear very bad indeed. ("The Delicate Balance of Terror," *Foreign Affairs*, Jan., 1959, p. 222.)

[7] Many of the implications of this relationship between the "strike first" and "strike second" outcomes are exposed in Wohlstetter's brilliant and authoritative article, *op. cit.* In part, the present approach is an attempt to formalize some of the propositions in Wohlstetter's discussion.

We will refer to $\bar{q}$, that probability of a U.S. Strike which would, with given Soviet pay-offs, make the Soviets indifferent between Strike and Wait, as the "critical risk" of a U.S. Strike. This threshhold expectation, defined as a function of Soviet pay-offs, seems a highly significant property of the pay-off structure. Among the most important consequences of military choices is their impact upon this parameter, which serves as an index of the sensitivity of the Soviet decision to their expectation of being struck.

Extreme vulnerability of the U.S. retaliatory force will imply a low Soviet critical risk. It leads to an extreme advantage of the "strike first" over the "strike second" outcome with $(v_{11} - v_{12})$ much greater than $(v_{11} - v_{21})$. With the resulting low q, the Soviets would find Strike preferable if they attached even moderate likelihood to a future U.S. Strike. This is clearly an undesirable state of affairs; a Soviet Strike could appear a rational response even to highly ambiguous indications of a U.S. attack, of the sort generated periodically by any warning system. Under the general objective of improving the "reliability of deterrence" it seems desirable to reduce the sensitivity of the Soviet decision to fluctuations in q; thus, it becomes a subgoal to increase the critical risk, $\bar{q}$.

The principal method of achieving high $\bar{q}$—implying that the Soviets will not prefer Strike to Wait unless they are very sure of a U.S. Strike —is to reduce the vulnerability of the U.S. retaliatory force by measures which do not improve markedly the U.S. "strike first" capability; e.g., the replacing of highly vulnerable weapons by Polaris submarines, airborne alert, hardened or land mobile missiles. As v_{21} is lowered relative to v_{12}, a situation is approached in which the Soviets would prefer Wait even if they were certain that the U.S. would attack ($\bar{q} = 1$, corresponding to $v_{21} = v_{12}$).[5]

A further subgoal, towards improving the reliability of deterrence and lowering p, is to lower q, the Soviet expectation of a U.S. Strike. Most military choices operate directly upon pay-offs, U.S. and Soviet, with indirect effects on expectations. Changes in U.S. pay-offs will influence q by affecting the Soviet image of the U.S. rational incentives to Strike. Just as p corresponds to the U.S. estimate of the reliability of U.S. deterrence, q is essentially the Soviet estimate of the reliability of Soviet deterrence. A way to lower q is to increase, in Soviet eyes, indices of the reliability of Soviet deterrence which are analogous to

[5] Conceivably, this result might be nailed home by making v_{11} appear worse than v_{12}; suppose that the Soviets were led to fear U.S. "annihilation tactics" with a large retaliatory force in case of a Soviet first strike, but were also made aware that the U.S. was preparing for a strictly countermilitary campaign, avoiding cities and aiming at quick cessation, if war should arise under any other circumstances. See Herman Kahn, *On Thermonuclear War* (Princeton, 1960), pp. 162-89.

indices of U.S. deterrence: to increase $U(\text{Wait}) - U(\text{Strike})$ in U.S. calculations; to increase $\bar{p} = \dfrac{u_{11} - u_{21}}{u_{11} - u_{12}}$, the U.S. "critical risk"; to lower p.[*] This adds two new criteria of choice (lowering p being already included) to our list of military subgoals.

Having presented some apparatus of analysis at this length, there is little space in which to apply it here. Let us consider one example, by now rather familiar. Suppose that, as is frequently done, lowering the Soviet "strike first" outcome v_{21} were taken as the only significant subgoal under the objective of improving the reliability of deterrence; and suppose it were proposed to achieve this by emplacing "soft," fixed, slow-reaction IRBM's in Europe. Like any increase in our inventory of offensive weapons, this move would tend to decrease v_{21}. But only a little; fixed IRBM's are subject to no-warning attack by large numbers of accurate, high-yield Soviet medium-range missiles and bombers, and their existence would have a small or negligible effect on the expected outcome of a well-planned Soviet Strike. Even so, if other effects were ignored, as they often are, the move could seem desirable on the basis of this one criterion.

However, if we ask the impact of this move upon the other factors in our schema, conflicts with other criteria are likely to emerge. The most marked effects of the innovation would probably be: (a) a sharp decrease in v_{12}, the Soviet "strike second" outcome; (b) a sharp increase in u_{21}, the U.S. "strike first" outcome. Neither of these effects, at first glance, might seem undesirable in themselves, to count as "costs." Yet the drop in v_{12} relative to v_{21} would imply a lower Soviet critical risk $\bar{q}$; it would take less assurance than before of a U.S. Strike to make a Soviet Strike seem preferable. And meanwhile, the actual Soviet expectation q might be increased; for the rise in u_{21} relative to u_{12} (which would change negligibly) would mean lower U.S. critical risk $\bar{p}$, and, for given p, a reduced interval $U(\text{Wait}) - U(\text{Strike})$, so that Soviet deterrence would appear less reliable than before.[10] Thus, the several criteria we have considered for the reliability of U.S. de-

[*] An interdependence between p and q emerges here; it has been ably explored under the heading, "The Reciprocal Fear of Surprise Attack," by Thomas Schelling, in *The Strategy of Conflict* (Harvard, 1960), pp. 207-29. I would suggest, without developing the point here, that this interaction is most significant when $\bar{p}$ and $\bar{q}$, the U.S. and Soviet critical risks, are both low.

[10] Herman Kahn has emphasized that such an improvement in u_{21} may significantly improve u_{11}, by deterring the Soviets from such acts short of all-out Strike as might "provoke" a U.S. first strike if the U.S. first strike outcome were sufficiently high. See Kahn, *op. cit.*, pp. 136-44 and *passim*. The objections, which I share, to such a policy of "Type II Deterrence" are too lengthy to discuss here. At any rate, note that this subgoal, if accepted, would in this case conflict with the various criteria of the deterrence of a Soviet Strike (Kahn's "Type I Deterrence").

terrence would indicate that this move might be associated with higher p than before.[11] Furthermore, Soviet recognitions of this effect could lead, via Schelling's "reciprocal fear of surprise attack," to higher q and a further upward pressure on p.

These results are to be contrasted to those of the measures mentioned earlier for reducing the vulnerability of the retalitory force (raising $\tilde{q}$ by reducing v_{21} relative to v_{12}); moreover, a complex of such measures may be designed to raise u_{12} much more sharply than u_{21}, thus providing "second strike insurance" against the failure of deterrence while at the same time raising $\tilde{p}$, increasing the reliability of Soviet deterrence and lowering Soviet fears of attack.

Other specific arms control, civil defense, and active defense measures may be examined in terms of our schema; their implications for the various criteria will depend upon their differential effects upon all of the factors discussed. The discovery, in a particular case, that the implications in terms of several of the criteria (subgoals) conflict is not a failure of the approach; on the contrary, it is a signal of the need for closer analysis in that case, for the weighing of criteria, or for the invention of new alternatives which avoid or alleviate the conflict.

It is clear that this simple framework cannot capture all the complexities of strategic choices. It is in no sense a machine for providing answers; at most, it is a machine for asking useful questions and for preliminary testing of alleged answers. As such, it can be helpful; simple as it is, it is far more flexible and complex than single-variable models implicit in much "literary" discussion. Unfortunately, there has been a historical tendency on the part of policy-makers to reject the aid of abstract frameworks of the present sort on the grounds that they are "too simplistic," and then to make practical decisions on the basis of much cruder, implicit models.

[11] This argument follows Wohlstetter, *op. cit.*, pp. 222-30, particularly p. 229.

MECHANICS OF SOME LIMITED DISARMAMENT MEASURES

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Introduction

This paper deals with one small aspect of the rapidly growing field of arms control. Arms control, in current terminology, is considerably broader than disarmament. It embraces all the problems ranging from total disarmament to the selective strengthening of armaments for the purpose of increasing the stability of mutual deterrence.[1] Problems of reducing the risk of war and of its devastation if it comes; the mechanics and tactics of negotiation; the politics and technology of inspection; the legal, military, and political aspects of enforcement and sanctions; and the hard detailed analysis of the military impact of specific arms-control measures—all are subjects of interest, research, and exposition.

Although the field has a growing theoretical literature relating arms control to military strategy in general and to deterrence in particular,[2] little has been written on arms limitation as a process relating to the economics of military power. The present paper is addressed to only one specific question: How can disarmament measures affect the economics of military power?

The method is to assume that the production of military capability has the same abstract characteristics as other forms of production and to examine the effects of two broad types of arms limitation upon production of such a capability—all at a rather remote level of abstraction. I simply beg the important question of defining military output or capacity; what is said would apply to such a simple concept of output as firepower in a specified context. Also, problems of symmetry are ignored; the analysis deals only with what happens to one side.

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[1] No attempt will be made here to comment on the literature or to provide a bibliography. The reader interested in getting a general view of the field would do well to begin with *Daedalus*, Fall, 1960, which is devoted entirely to the subject; and see also the reports, studies, and hearings of the Subcommittee on Disarmament of the Committee on Foreign Relations, U.S. Senate. The literature extends even to worries about how serious would be the consequences should disarmament really work. See, for example, Walter Millis, "The Peace Game," *Saturday Rev.*, Sept. 24, 1960, pp. 13 ff.

[2] Herman Kahn, "The Arms Race and Some of Its Hazards," *Daedalus*, Fall, 1960, pp. 744-80; Thomas C. Schelling, *The Strategy of Conflict* (Harvard Univ. Press, 1960); Jerome Weisner, "Comprehensive Arms-Limitation Systems," *Daedalus*, Fall, 1960, pp. 915-50.

For present purposes it appears useful to consider that disarmament measures increase the cost of armament; that is, decrease the military capability that can be attained with a given defense budget. (Looked at in this way it does not necessarily follow that disarmament is always a good thing or that it should be applied to all kinds of military capacity.) Individual disarmament measures include: (1) Banning specific kinds of weapon systems; e.g., eliminating from the (future) inventory bombardment satellites. (2) Restricting the size of inventory of specific kinds of weapons, or components of weapon systems; e.g., limiting the number of missiles, or military manpower, etc. (3) Restricting production; e.g., the production of nuclear weapons or of delivery vehicles.

This paper is concerned primarily with the first two classes of measures: proscription and inventory limitation. The former is logically only a limiting case of the latter, but treating them separately has some pedagogic merit. Everything said in this paper is in a sense trivial, but there are two reasons for saying it: the deductions, simple as they are, look interesting; and the literature does not typically take explicit account of the relationships explored. Although the deductions are interesting, it should be abundantly clear that an analytic exercise such as this is not a suitable basis for policy.

Proscription

The discussion will treat a single kind of capacity K_1 (the terms "capacity" and "capability" will be used interchangeably) and five weapon systems, $F_1 \ldots F_5$, each of which in combination with one or more of the others can contribute to K_1, but none of which alone can achieve any positive value of K_1 no matter how much of that weapon exists.

Where F_1 and F_2 are perfect complements, effective banning of one of them will preclude developing the capacity for accomplishing the mission in question (Figure 1). Proscription will be discussed first under the assumption that there is some significant degree of substitutability among factors.

Suppose that weapons systems F_1, F_3, or $\ldots F_m$ in combination with F_2 can produce various amounts of K_1, but that $F_3 \ldots _m$ are inferior to

F_1 in that for any value of F_2 and of $F_3 \ldots _m \dfrac{p_1}{c_1} > \dfrac{p_3}{c_3} > \ldots \dfrac{p_m}{c_m}$. Then

take as the point of departure the situation indicated in Figure 2. With a budget equal to $0 - F_2{}^4$, $K_1{}^3$ will be attained by combining $F_1{}^3$ and $F_2{}^1$, and $F_3 \ldots F_m$ will, of course, be excluded. In the event that F_1 is effectively proscribed, F_3 (being the most effective alternative) will be

FIGURE 1

substituted for it.[3] The presentation is simplified by assuming that one unit of each weapon system costs the same as a unit of any other. With this assumption it is possible to represent the elimination of F_1 as being equivalent to an increase in the price of F_1 to that of the best substitute, F_3. With F_1 excluded the transformation curve is shown as F_2^4, F_3^4.

[3] It is assumed throughout that each level of capacity is achieved in the most efficient possible way; that is, that capacity is maximized given the budgetary and technical constraints.

TERMS—ALL FIGURES

1. a_1 = Probability of finding dF_1.
2. c = Marginal cost.
3. F_i^1 = Input into a posture.
4. dF_1 = Increment of illegal inventory of F_1.
5. K_i^1 = Military capacity;
   Subscript designates the kind of military mission to which K applies, e.g., limited war, deterrence . . .;
   Exponent indicates the value of K_1,
   $K_1^2 > K_1^1$ means that K_1^2 constitutes more capability to perform mission one than does K_1^1.
6. p = Marginal productivity.

FIGURE 2

With no increase in budget for the mission, the achievable capacity is reduced to K_1^2. To retain the previous capacity would require an increase in budget to F_2^6.

If F_4 is markedly inferior to F_3, but is still the third best system to combine with F_2 to get get capacity K_1, proscription of both F_1 and F_3 would reduce the capacity for mission one, attainable with the original budget, to K_1^1.

It obviously follows that the effective proscription of an efficient weapon system increases the cost of achieving a specified level of military capacity. Further, and also obvious, the more inferior the best substitute the greater the arms-control effect of such proscription.

There are also some implications as to the sequence in which arms-control measures might be undertaken, or as to the sequence in which the pressures for arms-control measures might be exerted. Assume, as is sometimes asserted, that it is easier to ban an inefficient weapon system than an efficient one.[4] The arguments that it is desirable *now* to

[4] Hedley Bull. *The Control of the Arms Race, Disarmament and Arms Control in the Missile Age*, a Report to the 2nd Oxford Conference of the Institute for Strategic Studies, Sept. 23-25, 1960, Chap. 9, p. 8.

attack the nth country problem and to proscribe the bombardment satellite are examples. The notion appears to be that if there is little vested interest (for example, bureaucratic or military interest) in a particular weapon system, it is easier to eliminate it or that it is easier to preclude a system upon which military power is not presently dependent. The force of these arguments as they pertain to future arguments depends largely upon the rate at which the parties discount the future.

The point can be most readily made, I think, by suggesting a loose, hypothetical sequence of events. I hold no brief for this sequence but only hope that it is not so obviously foolish as to suggest that no plausible one could be developed which would illustrate the point. Suppose that F_1 is some missile system, and let F_3 be a chemical or biological weapon system. Suppose the possibility of eventually banning missiles is not to be ruled out, and suppose that the likelihood of banning them or chemical or biological weapons is inversely proportional to the military efficiency of each and directly proportional to some other factor, say, world public opinion. Given that F_3 is at present a relatively inefficient system, it is by assumption easy to ban. Suppose that is done. F_1, the missile system, is still legal, K_1^3 (Figure 2) can still be achieved with budget F_2^4, so that the exclusion of F_3 has had no arms-control effect. However, with F_3 eliminated, if subsequently a change in public opinion should make practical the proscription of F_1, then F_4 would be the best available system to combine with F_2 and for the previous budget only K_1^1 could be reached. Thus, under some conditions it is possible that a proscriptive measure which has no arms-control effect in itself has an important effect when combined with others. Specifically, the banning of an inefficient system may have an expected arms-control effect if (1) the system which is eliminated has some positive probability of subsequently becoming efficient, for example, through research and development, or (2) the system precluded would become efficient upon the proscription of a more efficient system, and the probability of (subsequently) precluding the efficient system is positive.

Inventory Limitation

A second kind of disarmament measure is the application of a ceiling on the size of the inventory of a particular kind of weapon.

Consider first the case of perfect complements, Figure 1. Reducing the inventory of F_1 from F_1^2 to F_1^1 will reduce the achievable capability to K_1^2, ignoring any substitutes for F_1. The minimum budget required is reduced from F_2^4 to F_2^3, and, so long as the ceiling on F_1 is effective, no increase in budget can re-establish the capability K_1^3.

If F_1 and F_2 are close substitutes, restricting the inventory of F_1 will
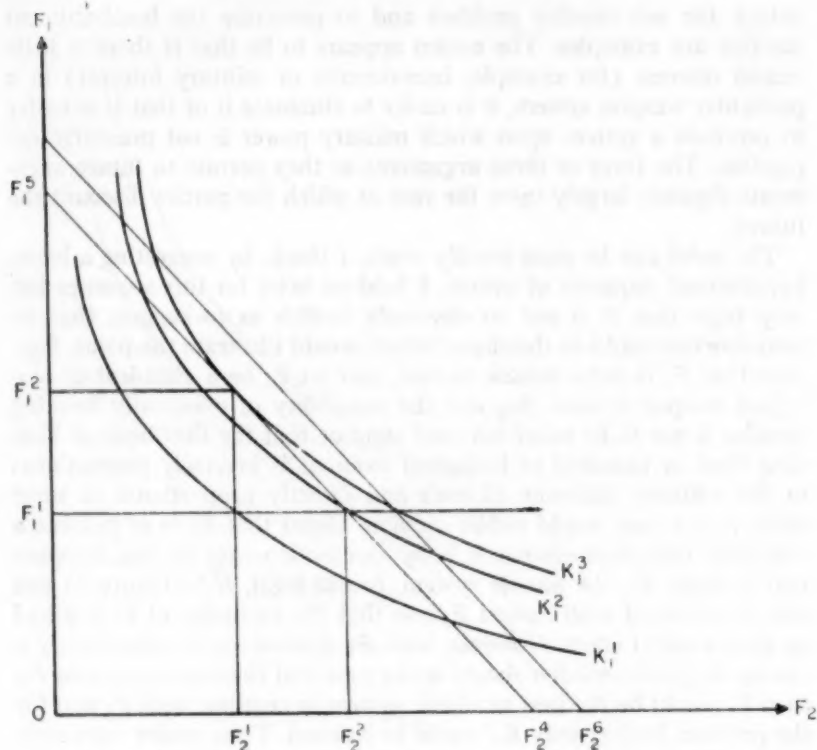
FIGURE 3

in itself have little arms-control effect as is shown by Figure 3. Reducing the legal inventory of F_1 to F_1^1 will reduce capability only to K_1^2 if there is no change in budget. With only a small increase in budget, to F_2^6, the original capability can be restored.

A corresponding reduction in F_2 alone would be similarly ineffectual. However, a reduction in F_1 accompanied by control on the inventory of F_2 looks more interesting. Suppose that with the reduction in F_1 there is also imposed a constraint that the inventory of F_2 cannot be increased. Maximum capability, at any budget, is then reduced to K_1^1 (Figure 3). Obviously, a joint reduction in both F_1 and F_2 would have still greater arms-control effect.

Thus, if systems F_1 and F_2 are not perfect complements and the probability of obtaining a limitation on the inventory of F_2 is greater than zero, the expected arms-control effect of reducing the inventory of F_1 is greater than the predicted arms-control effect of reducing F_1, while F_2 is free to vary.
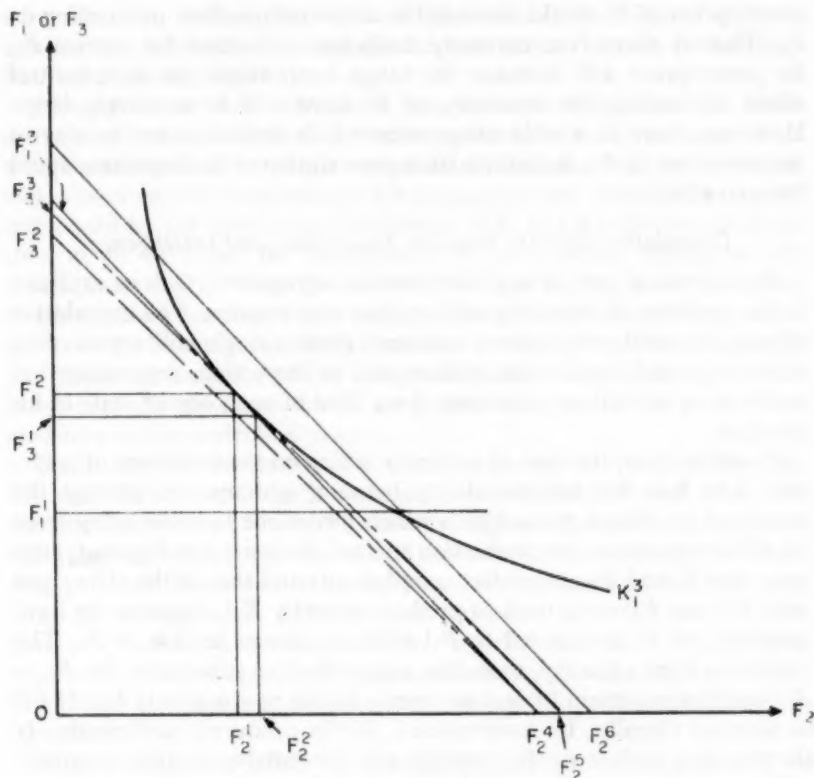
FIGURE 4

There is another way in which the availability of substitutes for F_1 influences the arms-control effect of restricting its inventory. Suppose that system F_3 is only slightly inferior to F_1 in that for any given amount of F_2 an expenditure on F_3 will produce less, but only slightly less, capability than would the same expenditure on F_1. This is illustrated in Figure 4 (compare the slopes of F_2^4/F_1^3 and F_2^4/F_3^2). With the same reduction in the allowable inventory of F_1 as that shown in Figure 3, capacity K_1^3 can be achieved at a budget expenditure of only F_2^5 (Figure 4). Thus, the arms-control effect of a restriction on F_1 alone is less than would have been the case in the absence of F_3, for without F_3 it would be necessary to spend F_2^6 to achieve K_1^3.

The significance of these relationships is clear. The existence of F_3 is of no consequence in this context if both F_1 and F_2 are unrestricted or if both are appropriately restricted. If for some reason it is possible now to restrict the inventory of F_1 but not of F_2, the prior

proscription of F_3 would increase the arms-control effect of a ceiling on F_1. Thus if there is a currently inefficient substitute for system F_1, its proscription will increase the range over which the arms-control effect of limiting the inventory of F_1 alone will be relatively large. However, there is a wide range over which limitation on the size of the inventory of F_3, as distinct from proscription of it altogether, would have no effect.

Cumulative Effects: Evasion, Inspection, and Intelligence

An important part of any disarmament agreement (tacit or explicit) is the problem of deterring and/or detecting evasion. The cumulative effects of several arms-control measures, given a single military mission, have reciprocal implications with regard to inspection, suggesting that under some conditions there may be a kind of economy of scale in inspection.

Consider, first, the case of setting a ceiling on the inventory of a system. The less the substitutability between systems, the greater the benefit of evading a proscription which forces one to move away from an efficient point on the production surface. Referring to Figure 1, suppose that F_1 and F_2 are perfect complements and tha* in the *status quo ante* $F_1{}^2$ and $F_2{}^2$ were used to produce capacity $K_1{}^3$. Suppose the legal inventory of F_1 is reduced to $F_1{}^1$ with no change in that of F_2. The maximum legal capacity, assuming away effective substitutes for F_1, is $K_1{}^2$ and the minimum budget necessary for its production is $F_2{}^3$. If $F_2{}^2$ is retained (legally, by assumption), $K_1{}^2$ is produced inefficiently. If the previous budget $F_2{}^4$ is (overtly, and presumably legally) retained, still more of system F_2 could be obtained. In any case in which more than $F_2{}^1$ of F_2 is preserved, concealing an increment of F_1 would provide a capacity greater than the legal capacity, $K_1{}^2$. Each unit of F_1 concealed (up to the efficient amount for combination with the "excess" stock of F_2) would have a high marginal productivity and presumably the covert increment of K_1 over and above $K_1{}^2$ might have peculiar military value through its element of secrecy or "surprise."

It is obvious that concealing stocks of a weapon for which there are close legal substitutes has relatively little advantage, *ceteris paribus*. In the case where a limitation has been placed on the stocks of one of two (or more) complementary systems the above is suggestive in terms of inspection and intelligence. With legal inventories of F_1 restricted to $F_1{}^1$, the existence of more than $F_2{}^1$ of F_2 might be viewed with suspicion and would suggest more intensive searching for (1) secret stocks of F_1 or (2) perpetrators of inefficiency in the defense establishment. Ignoring (2), this suggests that an agreement limiting the stocks of F_1 but not F_2 could be made more reliable by permitting inspection or other-

wise providing for keeping track of the stocks of F_2. Without such an agreement, particular intelligence interest should attach to keeping track of the inventory of F_2.

Although somewhat beyond the present framework, there are some interesting dynamic aspects of these problems. The effectiveness of an arms-control measure which is not violated, the advantages of violation and, therefore, the implications for inspection and intelligence are all influenced by the production lead-times of F_1 and F_2. Should the lead-time of F_1 be large, the termination of an inventory ceiling of F_1^1 would mean that a "long time" would be required to reach K_1^3 again. Similarly, the advantage of concealing and hence of finding illegal stocks of F_1 would be enhanced. It follows that for any given effectiveness of the inspection-sanction chain, the expected arms-control effect (cumulated over future time) would be greater the longer the production lead-time of the controlled weapon.

Continuing the discussion of limitations on inventories, suppose that the legal inventories of both systems have been reduced, to F_1^1 and F_2^1. Assume further that F_1 and F_2 are best hidden independently, but have the same costs of concealment and the same costs and mechanics of being found. These are rather extreme assumptions but they are not entirely unreasonable. Missiles and guidance systems, aircraft and bombs, missiles and warheads obtained by nth countries in co-operation with major powers appear to be plausible examples of end-items which fit at least the first two requirements; many kinds of production systems do.

Whatever the probability of finding illegal inventories of F_1 and F_2 (dF_1 and dF_2) alone, the probability of finding either dF_1 or dF_2 is greater. That is, if a_1 is the probability of finding dF_1 and a_2 is the probability of finding dF_2, the probability that both will go undetected is $(1-a_1)$ $(1-a_2)$, and the probability that capacity K_1^3 could be maintained covertly is only $(1-a_1)$ $(1-a_2)$. Thus, for example, in evaluating a proposal for the limitation of inventories of F_1 and F_2, a .2 probability that F_1 could be hidden and that K_1^3 could be clandestinely retained might be unacceptable; a risk of .04 that K_1^3 could be secretly maintained might be acceptable. The former might be the risk if only F_1 were limited, the latter if both were. However, the more divergent the methods of inspecting for contraband stocks of the two systems, the less the economies of inspection.

Conclusion

This suggests that although the broader the definition of weapon systems limited by an arms-control measure the greater its effect, if there is reason to believe that successive expansion in scope is possible

and, particularly, if one agreement is expected to make successive ones easier, the expected arms-control effect of a measure of narrow scope is greater than would otherwise be the case. "Narrow" arms-control measures may, under the restricted conditions assumed here, tend to be mutually reinforcing both in a simple sense and in that they may make concealment of capability disproportionately difficult. Finally, the fact that a weapon system is inefficient, and not used, does not necessarily mean that its proscription would have an expected arms-control effect of zero.

Some tentative implications for research are suggested. First, it appears desirable to explore the proposition that constraint of an inefficient (or future) system is easier to achieve than constraint of an efficient (or currently used) system. If the proposition is valid, it may be of some interest in terms of preparation for crash arms control.

Second, economists with military interests might find it useful to explore the arms-control implications of the fact that many kinds of military assets are joint factors. For example, military transport systems and military manpower are multipurpose resources. The effect on military capability of restricting the amount of such an asset as contrasted with control of a more specific asset is not obvious. For example, how would a manpower limitation as compared with the proscription of small atomic weapons affect the limited-war capacity of the West and of the Communist bloc?

Third, the dynamics of arms control raises some interesting economic problems. Given that the resources available for arms control have opportunity costs, how can they best be committed both statically and over time to reduce the danger of immediate war and to curtail the most dangerous aspects of the arms race? Expansion along the time dimension of the standard economic treatment of production processes might prove instructive.

Fourth, it looks promising to relate the kind of bargaining analysis developed for example by Schelling[8] to the theory of production in a military context.

[8] *Op. cit.*

ECONOMIC DEVELOPMENT IN MAINLAND CHINA

PRELIMINARY ESTIMATE OF THE NATIONAL INCOME OF THE CHINESE MAINLAND, 1952-59

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For the past several years, the authors have been engaged in a study of the national product of the Chinese Mainland for the years 1933 and 1952-57[1] An earlier paper[2] reported on some very early findings of that study. The present paper presents some revised and updated results of that same study now nearing completion and attempts a rather crude and conjectural estimate for the more recent and turbulent years 1958 and 1959.

The summary figures on net value-added by fourteen industrial sectors for the years 1933 and 1952-59 are shown in Table 1. Contributions to the domestic product by agriculture, manufacturing, mining, utilities, and modern communications and transportation are estimated by deducting values of intermediate products used in the process of production from values of output. Except government administration for which specific information is more or less available, value-added in the other sectors can be estimated only very roughly.

To work with Chinese Communist statistics, it is important to examine their plausibility and accuracy, making use of all direct and indirect evidence, without any prejudgment of the reliability of their data. During 1952-57, the Chinese Communists made serious efforts to improve their data collection system. On the other hand, the First Five Year Plan, while nominally beginning with 1953, was actually inaugurated during its third year, in 1955; and from then on the pressure exerted by the Communist regime on the managers of enterprises and local party members to fulfill or over-fulfill the assigned quotas increased steadily. The incentive and compulsion for local Communists to inflate their statistical reports increased as the drive to reach one target after another was intensified. These tendencies had varying effects on the reliability of different categories of Communist statistics.

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[1] A RAND research project. Chong Twanmo collaborated on some aspects of the estimate of the agricultural sector. During various stages of this research, the authors have benefited from suggestions of P. G. Clark, Hans Heymann, Jr., Oleg Hoeffding, J. A. Kershaw, Simon Kuznets and R. H. Moorsteen.

[2] "Structural Changes in the Economy of the Chinese Mainland, 1933 to 1952-57," *A. E. A. Papers and Proceedings*, May, 1959.

By 1958, when the "great leap forward" was taken, the pressure from Peiping to show sensational gains in production was so great that it more than offset whatever improvements had been made in the statistical reporting system. In spite of the poor quality of the Communist statistics, indications are that a most impressive expansion was achieved in producers' goods industries. But this expansion was brought about at a terrific cost in terms of the current standard of living.

TABLE 1

NATIONAL INCOME OF THE CHINESE MAINLAND, 1933 AND 1952–59*

(BILLIONS OF CONSTANT 1952 YUAN†)

	Our Preliminary Estimate of Domestic Product							Our Conjectural Estimate of Domestic Product	
	1933	1952	1953	1954	1955	1956	1957	1958	1959
Net value-added in:									
1. Agriculture	33.9	34.2	34.8	35.5	35.8	37.0	37.2	40	42
2. Manufacturing factories	3.3	6.5	8.3	9.6	10.4	14.3	16.0	19	25
Producers' goods	0.8	3.2	4.4	5.3	6.3	9.4	11.1	14	19
Consumers' goods	2.5	3.3	3.9	4.3	4.1	5.1	4.9	5	6
3. Handicraft	4.4	4.7	4.8	5.0	5.1	5.3	5.4	6	6
4. Mining	0.5	1.5	1.5	1.8	2.3	2.4	3.1	4	3
5. Utilities	0.1	0.3	0.4	0.4	0.5	0.6	0.7	1	1
6. Construction	1.0	1.8	2.3	2.7	2.9	5.0	4.6	6	8
7. Modern transportation and communications	1.1	2.1	2.6	2.9	3.1	3.5	3.8	4	5
8. Old-fashioned transportation	2.6	2.7	2.5	2.4	2.3	2.5	2.4	3	3
9. Trade	8.2	9.7	10.0	10.2	10.5	11.2	11.5	12	14
10. Government administration	1.4	3.5	3.7	4.0	4.1	4.8	5.0	5	6
11. Finance	0.4	1.3	1.3	1.5	1.5	1.7	1.8	2	2
12. Personal services	0.6	0.6	0.6	0.5	0.5	0.5	0.5	1	1
13. Residential rents	2.0	2.3	2.3	2.4	2.4	2.5	2.6	3	3
14. Work brigades		0.6	0.4	0.4	0.8	0.8	0.9	3	3
Net domestic product	59.5	71.4	75.3	79.3	82.3	92.1	95.3	108	125
	Communist Own Estimate of "National Income"								
	61.1	70.0	73.9	78.8	88.8	93.5	125.3		152.4

* Subitems do not necessarily sum up to totals because of rounding.
† Official Exchange Rate: 1 U.S. $ = 2.343 1952 Yuan (The "Jen Ming Pi").
NOTE: For well-known reasons, the temptation to convert the figures given in the table on the official exchange rate to those in terms of the U.S. dollar must be resisted.

I. *Preliminary Estimate for 1952-57*

The method of estimating domestic product by the value-added approach is straightforward. In making this estimate, however, important adjustments have had to be made in the original Communist data. Especially important are the adjustments of their statistics on food crops and industrial production.

Adjustment of Communist Data on Food Crop Production. An analysis of the Communist figures on food crop production suggests that their pre-1956 figures are underestimates of the actual output, while their 1958 and 1959 data are overestimates. This can be vividly seen from the data in Table 2, showing the calorie intake per head of population implied in their production data.

The prewar (1933) level of per capita calorie intake derived from food crops is only about 1,940 calories per day. Calorie intake from other sources was small, no more than 10 per cent of that from food crops. Taking account of the lack of protein and other protective foods, there was precious little room for reduction from the prewar calorie intake level without resulting in wholesale starvation. Yet, if the Communist data on food production for 1949-55, as reflected in the calorie intake figures shown, had been even roughly correct, the Chinese people would have endured a diet substantially below the starvation level for seven long years. There were, of course, many reports about shortages of food on the Mainland; but there was no evidence of starvation on a national scale. There are no indications that during those years the health of the Chinese people as a whole was impaired

TABLE 2

PER CAPITA CALORIE INTAKE DERIVED FROM FOOD CROPS PER DAY

| | Our Estimate 1933 | Implied in Communist Data on Food Crop Production | | | | | | | | | | |
		1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959
Number of calories per capita per day..	1,940	1,190	1,350	1,440	1,610	1,580	1,590	1,720	1,820	1,830	2,440	2,580

to a degree inevitable if the level of food production was actually as low as these figures would imply, especially considering the hard physical labor the Chinese people had to perform during these years.

For the years 1956 and 1957, we tentatively accept the Communist data on food crop production, as there is no convincing evidence to indicate that these figures have significantly underestimated or overestimated the actual output.

That they are not underestimates is suggested by the fact that the levels of per capita calorie intake per day implied in these output data are roughly 14 per cent higher than that implied in the estimated average ration allowed by the Communist regime.[3] It is reasonable to assume that the Communists, knowing that rationing cannot be enforced with 100 per cent effectiveness, would plan the ration at a level lower than the actual amount of consumption. But the rationing and control regulations were probably sufficiently effective to hold actual consumption within a 10 to 15 per cent margin of the rations provided.

Nor are the figures likely to be overestimates. The levels of per capita calorie intake derived from food crops per day, as implied in the Communist 1956 and 1957 data, are respectively 1,820 and 1,830 calories, or about 6 per cent lower than our prewar estimate of 1,940

[3] The average ration is estimated to yield about 1,600 calories per day.

calories. This is probably not far above the starvation line. Since there is no indication that wholesale starvation occurred in 1956 and 1957, it would appear that the Communist data for these years do not significantly overestimate the actual output.

It is exceedingly difficult to estimate the amount of production for the earlier years 1952-55; at best we can give only an educated guess. Considering the low level of consumption during 1956 and 1957, consumption of food crops on a per capita basis was unlikely to have been much lower during 1952-55, as there is no evidence that the general health conditions and working capacity of the people were poorer during these years. We can probably also rule out the possibility that per capita consumption of food crops was significantly higher in this period. Under these circumstances, the most reasonable hypothesis is that per capita consumption of food crops remained more or less constant at the 1957 level throughout the period 1952-57.

This assumption enables us to make a rough estimate of the production of food crops for all the years 1952-57 on the basis of the per capita consumption figure for 1957 and the data on the rate of change in population and on the proportions of food crops used for food purposes. This "backward projection" from 1957 to 1952 yields an output estimate for 1952 that is about equal to the 1933 production level. Since internal fighting ceased in 1949, it is reasonable to expect that by about 1952 agricultural production should have regained the prewar level.

Adjustment in Communist Data on Industrial Production. Starting with the official Communist figure on gross value of industrial production, we have attempted to estimate its physical composition in terms of the various commodities insofar as they can be accounted for. We have then considered the plausibility of the magnitude of the unaccounted-for residual. We were able to estimate with some confidence the value of output of twelve important categories of producers' goods (pig iron, steel, rolled steel, machinery, cement, sheet glass, construction materials, coke, chemicals, paper, gunny sacks, and auto tires) and fourteen of consumers' goods (cotton yarn, cotton cloth, silk, silk piece goods, woolen textiles, "grass cloth," clothing and knitted goods, sugar, milled rice, wheat flour, edible vegetable oils, cigarettes, matches, and rubber footwear). After subtracting the "identified" items from the official Communist totals on gross value of producers' goods and consumers' goods output, the shares of these total that remained "unidentified" during 1952-57 are presented in Table 3.

It is interesting to note that during 1952-57 the unidentified portion of producers' goods stayed within a fairly constant range of about 27-32

per cent of total gross value. However, the unidentified share of consumers' goods increased steadily from 26 per cent in 1933 to 42 per cent in 1957. During the same six years, the unidentified portion is reported to have increased by 200 per cent from 3.1 to 9.6 billion 1952 yuan (see first footnote, Table 1), whereas the identified portion increased only by 45 per cent from 9.1 to 13.2 billion yuan.

The identified portion of the consumers' goods includes in it all the major manufactured goods consumed by the Chinese people. With rationing of food and clothing strictly enforced, it is difficult to understand the reported much faster rate of increase of unidentified consumers' goods. The Communists themselves have published some ag-

TABLE 3

PROPORTIONS OF THE GROSS VALUES OF OUTPUT OF PRODUCERS' GOODS AND CONSUMERS'
GOODS "UNIDENTIFIED" BY PHYSICAL CONTENT (IN PER CENT
OF THE GROSS VALUE OF PRODUCTION)

	1933	1952	1953	1954	1955	1956	1957
Producers' goods....	56	27	30	31	32	27	30
Consumers' goods....	20	26	32	35	35	41	42

gregate data on the supply of "daily consumption items" from 1952 to 1956 which include such items as: china and earthenwares; consumers' metal products; leather and fur products; glass products; furnitures; soaps and cosmetic products; cultural, educational and "technical products"; and a big unnamed "others" category. The gross value of output of this aggregate group of consumers' goods increased from 3.7 billion yuan in 1952 to 5.3 billion yuan in 1956, an increase of 44 per cent. This rate of increase is consistent with that observed in the identified portion of consumers' goods, but bears no resemblance to the 200 per cent increase reported for the unidentified portion.

It may be argued that the unidentified portion consisted of goods which were not among the daily consumption requirements or are new products not previously produced. But it is not easy to guess what these goods might be, other than those covered in the identified portion and in the several categories of daily consumption items just discussed. At the risk of duplicating some of the items already covered in the daily consumption items, we have compiled some fragmentary output data on fountain pens, radios, clocks, hot water bottles, pencils, bicycles, antibiotics, books and magazines. The total value of these goods is not likely to exceed 1.1 billion yuan in 1957. Contrasting this figure with a total value of 9.6 billion yuan worth of unidentified consumers' goods for 1957, one is inclined to conclude that, however fast

the rate of increase of the "luxury" items enumerated above may have been, the increased production of such items could hardly add up to the impressive size of the unidentified share of the gross value of consumers' goods by 1957. Since there is a strong presumption of a major exaggeration in this item, we have more or less arbitrarily assumed, in estimating the contribution by manufacturing factories to the domestic product, that the unidentified consumers' goods increased over this period at the same rate as the identified portion; i.e., by 45 per cent from 1952 to 1957.

Rates of Growth. During 1952-57, our calculations show that the average annual rate of growth of net domestic product was 6 per cent per year in constant 1952 prices. Estimates of domestic product have also been made on constant 1933 and 1957 prices; the average rates of growth are, respectively, 4.4 and 5.7 per cent. The average annual rate of growth of the producers' goods industries as a whole (including the producers' goods portion of the output of manufacturing factories, the construction industry, the utilities and the mining industries—see Table 1) is much higher: 24 per cent per year. While this estimate may still include elements of upward bias on account of the well-known "new product" and aggregation effects and the tendency of local Communists to inflate their statistical reports under pressure from Peiping to fulfill and overfulfill quotas, the rate of growth would still be very high even if the estimate were adjusted downward, say, by as much as 30 per cent. Such an impressive rate of expansion of the producers' goods industries was achieved only at a terrific cost in terms of the living standards. According to our estimate of domestic expenditure by end use, per capita consumption in 1957 was still 11 per cent below the meager 1933 level.

II. *Conjectural Estimate for 1958-59*

Regulation and control of economic activities were progressively and drastically tightened during the period 1952-57. The severity of the control measures introduced during those years, however, was mild indeed when compared with the developments in 1958. Communes were introduced in 1958. Within a few months, the entire rural population was organized into 26,000 communes, most of which were almost indistinguishable from army camps. In addition to providing an instrument for total political control of the population, the communes afforded a means of integrating agriculture and industry into one gigantic machine that could utilize all human labor resources (including housewives) to the maximum. On top of the continued emphasis on modern and capital-intensive industrial development, a massive effort was made to increase

production by using labor-intensive techniques on crude equipment. Backyard blast furnaces were built to produce steel in cities and in villages, and such techniques as deep ploughing and close planting were employed in agriculture. All these efforts, in industry and in agriculture, were to bring about a "great leap forward" in 1958, generally aiming at a doubling of output in one year from 1957 to 1958.

With the announcement of the great leap forward in December, 1957, the central Communist regime in Peiping exerted tremendous pressure on local party members, directors of communes, and managers of local enterprises to expand production at a pace which was practically impossible to achieve. What was achieved in a few pilot projects by concentrated use of technical skill and scarce resources under the most favorable conditions and closest supervision was expected to be duplicated by producing units all over the country. Soon enthusiastic reports were pouring into Peiping from one locality after another, claiming that the targets were being fulfilled and overfulfilled. In April, 1959, the Communist regime announced that the output of food crops and steel more than doubled from 1957 to 1958 and that the gross values of agriculture and industrial production both increased by 75 per cent.

It soon became apparent that these announced increases could not be correct, as there were neither improvements in food rations nor evidence of sufficient increases in the supply of industrial goods to sustain the claim of 65 to 100 per cent increases in output. A drastic downward revision of the claims was announced in August, 1959, reducing the estimated production in 1958 by one-third. The claimed increase in steel production was scaled down in a more subtle way. It was admitted that roughly 30 per cent of the steel produced in 1958 was of the "native" kind, not really usable for modern industrial purposes. Since the total quantity of steel produced was not really reduced in the revised announcement, the total value of industrial production remained unchanged. The increase in agricultural production announced for 1959 was more restrained, but those claimed for steel and industrial production in general were very large.

Communist production statistics for 1957-59, including the reported percentage increases in national income, are summarized in Table 4.

Even after the drastic downward revision, the increases in production reported for 1958 and 1959 remain large. In dealing with the earlier Communist data for 1952-57, we detected certain apparent biases and made some bold but relatively limited adjustments in an effort to correct them. The orders of magnitude of our corrections are indicated by the downward adjustment in the rate of growth of na-

tional income that results. A 9 per cent average annual rate of growth, claimed by the Communists, was reduced to 6 per cent by our calculations. For 1958 and 1959, however, the Communist claims of increases in national income are 34 per cent and 22 per cent, respectively. Confronted with such magnitudes and such a sharp discontinuity in Mainland China's economic development, we can hardly expect to be able to use the same methods of limited adjustment that we relied on in the earlier years. Obviously much more drastic and heroic measures are required. The results are correspondingly of more doubtful significance, and the best that can be said for our conjecture estimate is that it is

TABLE 4

COMMUNIST PRODUCTION DATA FOR 1957–59

	1957	1958		1959
		April 1959 Announcement	August 1959 Announcement	
Food crops production (million tons)....................	185	375	250	270
Gross value of agricultural production (billion 1957 yuan)........	54	88	67	78
Steel production (million tons) "Modern type steel"...........	5.4	11.1	8.0	13.4
"Native type steel"...........			3.1	
Gross value of industrial production (billion 1957 yuan)........	70	117	117	163
Percentage increase of national income over the preceding year...	+ 5.4%	+34%		+ 22%

in all likelihood nearer to the actual magnitudes than the highly inflated Communist claims. Only the principal adjustments we have made in the Communist data can be described here.

1. Since there have been no announced increases in rations or significant increases in exports or additions to stock in 1958 and 1959, food crop production is assumed to have increased from 1957 to 1958 and 1959 at rates equal to those of population growth during these years. However, reports about shortages of food supply in the cities during 1958 and 1959 became more numerous than before. Food consumption in rural areas was also more tightly controlled after the advent of the communes. The assumption we have made may still somewhat overestimate the rate of increase in agricultural production that actually occurred.

2. Unlike their producers' goods figures for 1952-57, the Communist

data on steel output for 1958 and 1959 are admittedly confused. In addition to the reclassification of about one-third of the steel output in 1958 as native steel unsuitable for modern industrial uses, the Communists disclosed more recently that even of the "modern type steel" produced in 1959, about 36 per cent was produced by "middle and small size converters" and that these middle and small size converters had become important during 1958 and 1959. One wonders whether during the enthusiastic "leaping" years the demarcation line between the small size converters and the backyard blast furnaces could have been clearly drawn. In making national income estimates, it is important to distinguish that part of steel output which was in fact steel from that part which was produced by the small size converters and backyard blast furnaces from scrap collected from the people in the form of pots and pans and which were again returned to them as pots and pans.

Rates of increase of steel production as high as 50 and 67 per cent (see Table 4) per year remain unpersuasive without some corroborating evidence in the form of comparable increases in steel products, which are essentially made of "modern steel." The only category of such products on which a continuous series of data is availabe for all the years 1952-58 is the value of machinery produced in constant 1952 prices. It is perhaps significant that machinery production had an almost perfect linear regression relationship with steel output during 1952-57; but the 1958 observation of steel lies greatly above the relationship. On the basis of this admittedly inadequate evidence, we tentatively estimate the production of bona fide steel in 1958 in accordance with this linear relationship and the value of machinery produced in 1958. The excess of the Communist figure for 1958 modern type steel over our estimate is assumed to be that part of steel produced from the middle and small size converters which should have been classified as handicraft output, and it has been so dealt with in our estimate. No Communist report on the value of machinery produced in 1959 is as yet available for making adjustment in the output of steel claimed for that year. Their figure on modern type steel for 1959 is therefore tentatively divided into modern industrial steel and a part which is not on the estimated ratio for 1958 discussed above.

3. The gross value of output of manufacturing factories in 1958 and 1959 is again separated into an identified and an unidentified portion, and the rate of growth of the unidentified portion is assumed to be the same as the identified portion. The method used here is the same as that applied to the 1952-57 data.

After these major and some other minor adjustments have been

made, the conjectural estimates of domestic product for 1958 and 1959 presented in Table 1 are obtained in essentially the same way as the 1952-57 estimates. The rates of growth of the domestic product so estimated are 14 per cent and 15 per cent, respectively, for 1957-58 and 1958-59. Though much lower than the corresponding Communist figures (35 per cent and 22 per cent, respectively), they still appear extraordinarily and unpersuasively high. More confidence can perhaps be placed in the estimated rates of growth of the value-added in the producers' goods manufacturing factories in 1958 and 1959. (See Table 1.) The average rate of increase during 1957-58 and 1958-59 comes to 32 per cent per year, about 10 per cent higher than the average rate of 29 per cent for the 1952-57 period as a whole.

COMMUNIST CHINA'S STATISTICAL SYSTEM: 1949-57[*]

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One of the most baffling problems in Chinese economic studies is the validity of official statistics. The development of research in this field—and in some quarters even the justification of the studies as a field—depends on the answer. Since official admission of gross exaggerations in the 1958 statistics, the problem has become all the more acute. In the discussion of claims and counterclaims, appeals to common sense are not convincing. There is a dire need to search for substantial evidence with which to form a well-grounded judgment.

This paper, dealing with the statistical system of Communist China, will not concern itself with statistical definitions and concepts, nor will it enter into an examination of official statistics. Both of these approaches some of us have already taken elsewhere.[1] Rather, it will attempt to appraise the quality of official statistics through a study of the development and the inner working of the system. This will involve a painstaking investigation, and we should be prepared to go through a mass of significant details without which conclusions cannot be justifiably drawn. Because of the length of the study, it is necessary to divide it into two parts, one—the province of the present paper—covering the period from 1949 to 1957 and the other from 1958 to 1960. Moreover, this occasion permits the presentation of the main conclusions only; publication of the full text must be deferred.

A word should be said about the sources of information. This study relies heavily on the information given in the complete set of official journals of the State Statistical Bureau of Peiping, published between April, 1954, and February, 1959. Designed primarily for communicational and training purposes within the national statistical services, they were not made available to the public until after the "free discussion period" in late 1957, when current issues were allowed to be exported. Since February, 1959, they have again been withdrawn completely from public circulation.[2]

[*] This is a paper from the research project on the Agriculture of Communist China.

[1] See, for example, my *Economic Development of Communist China* (Univ. of California Press, 1959), and "Economic Development," *The China Quar.*, Jan.-Mar., 1960.

[2] The State Bureau first published its journal in Apr., 1954, as a monthly, under the title *T'ung-chi kung-tso t'ung-hsin* ("Statistical Bulletin"). It was changed into a semimonthly in 1956, and renamed *T'ung-chi kung-tso* ("Statistical Work") in 1957. In the first nine months of 1958 the State Bureau also published a monthly, *T'ung-chi yen-chiu* ("Statistical Research"); after the Sept. issue it was merged into the *Statistical Work*. In Jan., 1959, the

From 1949 to 1952 no effort was made to develop a statistical system national in scope. The emphasis in statistical work was placed on the state sector, especially state and joint industrial enterprises, for which a national survey was undertaken as of December, 1949. In agriculture, the land redistribution program probably resulted in improving the data on cultivated acreage and rural population; as regards sown area and output, reports were made by the *hsien* governments at their own will. National aggregates for this period were estimates based on a small number of unorganized local surveys, which could not be used for compiling national statistics.

A national statistical system started with the operation of the State Statistical Bureau in October, 1952. According to the report of an Indian delegation that went to China in the summer of 1956, it was organized chiefly along the line of functional statistics into 15 departments, with 611 technical and 64 nontechnical staff members, headed by a director who was an economist and 5 deputies, of whom 2 were statisticians and 3 were economists.[3] It was larger than most of the central government ministries.

What were the objectives and principles that had guided the State Bureau's work from the beginning? These are revealed in its directives and pronouncements. First, in order to promote and facilitate the compilation of national statistics, "a centralized and unified statistical system is to be created, in which the State Bureau will be responsible for organizing all the work in statistics in the country, for standardizing methods of checking accuracy and computation, and for centralizing the distribution of all basic statistical schedules."[4] The Soviet "pace-setting experience" would be followed and should be carefully studied by all the rank and file in statistical services.[5] Second, statistical offices were to be established at all levels of local government within each province. While organizationally a part of the local governments, operationally they formed a more or less independent network with the State Bureau as the highest authority. The primary purpose of this

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journal combined with *Chi-hua ching-chi* ("Planned Economy"), the official monthly of the State Planning Commission since Jan., 1955, to become *Chi-hua yu t'ung-chi* ("Planning and Statistics"). Another important source is the official newspaper, *Jen-min jih-pao* ("People's Daily"). Direct quotations are my translations.

[3] The fifteen departments were: statistics of industry, agriculture, basic construction, trade, distribution of materials, transportation and communication, labor and wages, culture and education, and health, comprehensive statistics, research, editing and translating, machine calculation, and general affairs. See Government of India, Ministry of Food and Agriculture, *Report of Indian Delegation to China on Agricultural Planning and Techniques* (New Delhi, 1956), pp. 82-83. Hereafter cited as *Indian Delegation Report*. It may be added that over the years the number of deputy directors had varied from three to five, there being four since Jan., 1958.

[4] "Director Hsieh's Report at the Third National Statistical Conference," *TCKTTH*, 1:4-11, Apr., 1954.

[5] "The National Conference on Statistical Work," *JMJP*, Dec. 29, 1952.

national organization was "to render service to the task of planning" at all levels.⁶ Third, statistical data must be "both accurate and reported on time."⁷ In addition, discrepant figures must be reconciled so that only one set of basic statistics would be recognized as authentic.⁸ Fourth, statistical findings must be put to use. Their practical application during the period of national construction was fourfold: (a) as the basis for the preparation of national economic plans and a tool for supervision and inspection of their fulfillment; (b) as a powerful aid to state leadership in the economy, especially in respect to the state control of the nonsocialist sectors and to the formulation of socializing policies; (c) as the organizer in national economic accounting, since the demand for accuracy and punctuality in statistical services would entail improvement of accounting services and business records in the same direction; and (d) as the publicizing agent for pacesetters.⁹ For all this application, mere collection and compilation of data were not enough; the final data must be studied and analyzed.¹⁰ Through such an effort by the rank and file, the State Bureau aspired to uncover "the laws of national economic development."¹¹ Lastly, the training of statistical personnel was urgent. The program had to be carried out on a national scale and in such a way that a large working force would be produced within a short period of time.

These objectives were ambitious. Their implementation had encountered great difficulties. In fact, many of the underlying principles were severely criticized toward the end of 1957 and in 1958, and the whole statistical system was substantially revised in 1958 and 1959. How much had been achieved before the revision?

Throughout the period from 1952 to 1957, the State Bureau received full support from the central authorities of the Party and the government, with authority (a) to develop a national organization of its own, (b) to centralize the control over formulation and distribution of statistical schedules, (c) to establish a unified statistical-computation checking system, (d) to centralize the control and supply of basic statistics, and (e) to handle national censuses and sample surveys. How successful the State Bureau had been in attaining these objectives

⁶ Editorial, "To Further Strengthen Statistical Work in the Period of Economic Construction," *JMJP*, Mar. 31, 1954.

⁷ Hsu Chien, "To Service Planning Is the Basic Duty of Statistical Work," *TCKTTH*, 1:17-19, Apr., 1954.

⁸ Hsieh Mu-ch'iao, "Final Report at the Fourth National Statistical Conference," *TCKTTH*, 5:1-7, May, 1955.

⁹ Chiang Chao, "What Are Statistics and Their Functions?" *TCKTTH*, 2:27-31, May, 1954.

¹⁰ State Statistical Bureau, "A Directive concerning the Development of Analytical Work on Industrial Statistics," *TCKTTH*, 6:4-6, Sept., 1954.

¹¹ "A Record of Director Hsieh's Report at the Meeting of All Bureau Workers," *TCKT*, 6:1-6, Mar., 1957.

and when success or failure occurred provide an important basis for evaluation of the validity of official statistics published in different years.

In organization, provincial and city statistical bureaus were set up in 1953 and began functioning toward the end of the year. The drive to establish units in all special districts and *hsien* began the following year and in towns and *hsiang* in 1955 and 1956.[12] Only the effort at the special district level was successful (in 1954). The key to rural areas was the *hsien* where the State Bureau met with virtually complete failure. Thus, throughout the whole period, the entire agricultural sector was served merely by a small number of part-time statistical workers.

The State Bureau did not engage in direct field investigation for data but relied on the "business affairs" system for supply by means of regular schedules.[13] The statistical services in the controlling agencies and enterprises of the system, therefore, were of great importance to the State Bureau. In 1953, it began to assist the controlling agencies in setting up the services, which soon operated well enough in 1954 to enable the State Bureau to prepare and issue in September of that year the first statistical communiqués on the economy. Also in 1953 it started helping state and joint enterprises build up their statistical services. Success came first from the centrally controlled enterprises about September, 1954. It was reported that services had been established in all primary establishments by 1957. Hence statistical coverage rapidly widened after 1954. However, the keeping of accurate primary records remained a serious problem with all of them.

With regard to private industry and trade, the large-sized enterprises in some twenty cities were brought into the regular schedules system in 1953. But reports were found unrealistic and dishonest, nor could they be used to estimate the figures for other private concerns left out of the system. This led to a national survey of individual craftsmen and private industrial enterprises employing over ten persons in 1954 and to another of private trade and food-and-drink catering establishments in 1955. When private industry and trade were completely socialized in 1955-56, the State Bureau began to readjust its records concerning this sector for each year back to 1952. As regards agriculture, regular

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[12] Before the advent of people's communes in the summer of 1958, the administrative structure comprised the following levels in the order of decreasing scope of authority: the center, provinces and the equivalent, special districts (*chuan ch'u*) and autonomous divisions (*tzu-chih chou*), cities and *hsien*, districts (*ch'u*), towns and *hsiang*, and finally, villages. Thus, there were six tiers at the provincial level and below.

[13] The central ministries and their local counterparts that are concerned with the operation of state and joint enterprises, together with the people's bank and the tax bureaus of the Ministry of Finance, constitute the so-called "business affairs system" from which all economic statistics originate.

schedules were sent to the provincial governments from 1953 to 1957 with the understanding that the local governments below the provincial level would conduct field investigations before making the returns. After the establishment of statistical units at the special district level in 1954, statistical committees began to be organized in many rural areas for the purpose in 1955. But in reality reports on sown area and output of different crops were made by cadres in the *hsiang* and villages according to their own observation. If investigation was made, the method of "model survey" was most commonly employed, in which samples were chosen at the will of the investigator, chiefly on the basis of ease of obtaining data. Moreover, reports were invariably readjusted by Party and political leaders at every level of local government, where the conflict of interests had to be reconciled between the production bureau who tended to inflate the figures and the bureaus of tax collection and internal trade who tended to do the opposite. Collectivization in 1956 and 1957 did not contribute any improvement to the mechanism of statistical reporting, but rather produced a predominantly upward bias on the part of local leadership in editing the figures. Thus the agricultural statistics of 1955 were probably the least unsatisfactory for the whole period from 1949 to 1957. Nevertheless, all agricultural statistics were so poor that the director of the State Bureau had to admit at the end of 1957 that such important questions as the size of territorial area and cultivated land remained unanswered.

The State Bureau attempted to exercise quality control through several devices. The unified system of regular schedules, introduced in 1953, embodied the principle of complete count that the State Bureau tried hard to put into effect as far as possible. The system entailed the development of a set of indicators to be adopted in all schedules with standardized methods of computation and classification. The planning and the central and provincial "business affairs" authorities had been consistently using different schedules with diverse definitions and classifications. Difficulties with the planning authorities were not resolved until after the middle of 1955. But, because of their own operational needs, the business affairs ministries, bureaus, and departments were reluctant to comply. The volume of schedules bore heavily on all primary establishments and confounded the statistical workers in regard to proper computation and entry. Not only inaccuracy resulted, but the figures so produced could not be used for compiling national statistics. However, the authority of centralizing the formulation and distribution of all schedules could not be effectively enforced by the Bureau. Centralization was partially achieved for 1955 and 1956 at the central and provincial levels. In 1957 the business affairs ministries regained their authority to formulate and distribute their own

schedules. In rural areas control was found impossible; the flow of schedules and questionnaires turned into a deluge in 1957. The confusion in statistical schedules was compounded by the difficulties arising from the lack of standardization in weights and measures.

Another device for quality control was the unified statistical-computation checking system that required uniformity in the method of computing national indicators for statistical, financial, and operation records. For uniformity in statistical recording the method of double channeling of statistical returns was introduced in 1953. It took time for the method to be put into effect. Even in 1954 a large number of state and joint enterprises failed to comply. Since then, however, the scope of application widened. While wide application of this method resulted in some improvement in the quality of statistics, the scheme provided no effective check on recording in primary establishments, and also enabled the controlling agencies to adjust the figures according to their judgment. Uniformity in computation methods for statistical, financial, and operation accounts began in 1956.

Of all the devices for quality control, the one that had been most effectively implemented was unification of statistical figures. From 1949 to 1954 widely discrepant figures were produced and employed by the statistical, planning, and business affairs agencies on the same level and between different levels of government. None of these figures were recognized as "the authentic" or "the official." To avoid controversy and perhaps also because of the policy in early years to regard statistical figures as state secrets, only percentages were used in public statements. These ratios, however, were so inconsistent and contradictory that observers during this period could not help entertaining grave doubt about the validity of official statistics, raising the question whether there were not two sets of statistics, one fabricated for public consumption and the other corrected for planning. In fact, there were almost as many sets of statistics as data-collecting units in the central and local governments.

In March, 1954, the State Bureau was authorized to reconcile all statistical figures in basic national statistics. This was first achieved in industrial statistics before the end of the year and generally accomplished in the first half of 1955. Thereafter, official statistics began to be published in quantity, perceptibly improved in quality. Unification of statistical figures was also extended to publications, making those figures appearing in newspaper editorials, dispatches, public statements by officials, periodicals and books, as official as those issued by the State Bureau.

Centralizing the supply of basic statistics to the planning authorities —an authority given to the State Bureau also in March, 1954—was

much more difficult, as the planning authorities could obtain data from the business affairs system more promptly than through the state statistical organization. Only with the the full support of the State Planning Commission did the State Bureau attain reasonable success in this centralization in 1955.

Statistical personnel formed the weakest link in the national statistical front. The working force increased from 100,000 in 1953 to nearly 200,000 in 1957. An overwhelming majority of them never had any training in statistics or knowledge in statistical work. Statistical services were not able to recruit young people with ability, most new trainees in 1956 being old in average age, deficient in memorizing power. Morale had been very low among the working force, only to begin to improve in 1956. Thus statistical errors, falsification of data, and fictitious reporting were frequently committed, especially before 1956. It was claimed that the training program, conducted by the state statistical organization and the business affairs agencies, had given instruction to "several tens of thousands" through 1957. If so, at least one-half of the working force received no formal training, except learning on the job. Moreover, judging from the nature of the program, one may infer that the working force on the whole was equipped with not much more than some knowledge of filling out statistical schedules.

By putting all these developments together from 1949 to 1957, the year 1954 emerges as the dividing line in regard to the reliability of official statistics. In contrast to the purely internal statistics of the business affairs system, official statistics (or "national economic statistics" or "basic statistics") are a collection of national indicators, specifically defined, to be computed by standardized methods. (They do not cover the military.) All the percentages currently released before 1954 cannot be properly regarded as calculations from official statistics. In 1954, however, the state statistical organization was extended from the center practically to its limit; namely: the special districts; statistical services in the business affairs controlling agencies and in the centrally controlled enterprises began to function; the method of double channeling of statistical returns started to widen its application; a national census was taken of craftsmen and private industrial enterprises employing over ten persons. For the first time, the State Bureau was able to issue statistical summaries of the economy. And the use of telegraphic monthly and quarterly returns became effective in checking the major state and joint enterprises on their implementation of the state plan. It is clear that although established in the late summer of 1952, the State Bureau made its influence felt nationally for the first time in 1954.

The official statistics for the first five years, from 1949 to 1953,

released in late 1955, are, on the whole, poor estimates, with the exception of the statistics for state and joint industrial enterprises that had varying degrees of reliability. It may be recalled that the first state plan for industrial rehabilitation and construction was based on a national survey of state and joint enterprises of 1949, and the 1952 state economic plan, on the returns from major state and joint enterprises in industry, internal trade and agriculture. In the first two years of operation, the first five-year plan was in fact a plan of targets in percentages without real data. It was during these two years that the newly established provincial and city statistical bureaus made a concerted effort to search for and collect statistical materials for 1952, the base year of the plan. The base-period figures were finally prepared by the State Bureau in late 1954, although they were physical-output and value-product aggregates without detailed breakdowns.[14]

From 1954 onwards, on the other hand, the quality of official statistics improved and statistical coverage broadened. In 1955, definitions, classifications, and computing methods regarding national indicators were made uniform between statistics and planning. More important was the unification of statistical figures between the state statistical organization and the business affairs system, producing "final" figures for each year back to 1952. Even in agriculture, as has been said, some semblance of control was installed for the first time in the form of statistical committees; and the first of a series of national surveys of family budgets and of agricultural producers' co-operatives was initiated. It was also in 1955 that the State Bureau began to "compute" national income instead of "making a preliminary estimate" as in 1953. In 1956, primary records of erstwhile private industry and trade were made available for the first time to the state statistical organization, so that the early official statistics might be properly adjusted. Clearly it was due to the increased reliability of data and the widening of statistical coverage that the work program of the state statistical services for 1956 and 1957 called for processing all the statistical materials for important indicators since 1949 as well as those of 1936 in order to make them comparable year to year and also comparable to those of other people's democracies.

Needless to say, such gradual improvement in reliability and accuracy was only relative. The quality of statistics varies greatly from field to field. In early 1955, the director of the State Bureau gave quite an objective analysis of comparative reliability as follows: (a) in

[14] State Statistical Bureau, Department of Industrial Statistics, "Conclusions from the Industrial Statistical Work of 1954, and the Work Program for 1955," *TCKTTH*, 9:1-7, Dec., 1954; and "A Record of Director Hsieh's Report at the Meeting of All Bureau Workers," *ibid.*

terms of functional fields, industry was fair; trade, worse; agriculture, worst; (b) in terms of sectors, the state sector was fair; the capitalist sector, worse; and the individual sector (craftsmen and family agriculture), worst; (c) in the state sector, the locally controlled enterprises were worse than those centrally controlled; and nonbasic activities (such as industrial statistics of nonindustry ministries, trade statitics of nontrade ministries) were much worse than basic activities; and (d) in terms of indicators, physical output and value product were fair; labor and wages, worse; finance and cost, worst.[15] At the end of 1955, the State Bureau in an editorial commented further on the relative quality of different functional statistics by dividing them into two groups according to the strength of their foundation. The first group, having a fair foundation, included, in a decreasing order of strength, industry, transportation and communication, trade, and basic construction. In industry, better than others were the statistics of state enterprises, which, however, were confined to those for value product, physical output and labor force; statistics for such indicators as trial-manufacturing, output quota, and utilization of equipment had not been satisfactorily computed, if at all. The second group, having a weak foundation, comprised, in an increasing order of weakness, material allocation, culture, education and health, population, finance and cost, labor and wages, and agriculture. In agricultural statistics, the weakest were those on sown area, cultivated acreage, and production, by crops, according to economic classes; on size and production of livestock; etc.[16] Complete socialization of private enterprises and agriculture in 1956 and 1957 does not affect these conclusions on the relative reliability of different types of statistics.

In spite of all the State Bureau's effort at building up a state statistical system since 1952, the best type of statistics was yet authoritatively rated as merely "fair." Why is it that none could be rated as "good"? The answer clearly lies in such factors as low quality of the statistical working force, disorderliness in primary records, poor management in enterprises, confusion in the weights and measures system, and lack of standardization equipment. All these will take time to improve and, indeed, are none other than a manifestation of underdevelopment of the country.

[15] "Director Hsieh's Report at the Fourth National Statistical Conference," *ibid.* Most probably, the state sector was meant to include joint enterprises.
[16] Editorial, "The Need to Perform Statistical Services Dil'gently, Fast, Well, and Economically in Order to Meet National Construction Needs," *TCKTTH*, 2:3-6, Jan., 1956.

THE STRATEGY OF ECONOMIC DEVELOPMENT
IN COMMUNIST CHINA

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The central thesis of this paper is that during the first five-year plan period (1953-57) Chinese Communist policy-makers pursued a Stalinist strategy of economic development with local adaptations. However, given the vastly different factor endowments of Mainland China in the fifties as compared to the Soviet Union of the twenties, Chinese planners were forced to modify significantly their original approach. They thus evolved a new strategy for the second five-year plan (1958-62), based on intensive utilization of underemployed labor combined with promotion of technological dualism, as a means of maximizing the rate of economic growth. In effect, then, building on Stalinist foundations, they adopted an essentially Nurkse *cum* Eckaus model of economic development.

I

High rates of saving and investment institutionalized through agricultural collectivization, preponderant emphasis on the development of raw materials producing and investment goods industries, reliance on large-scale and capital-intensive technology in industry, relative neglect of investment in agriculture, in consumers' goods industries, and in social overhead may be said to represent the principal features of the Stalinist prescription.[1] In effect it is a pattern of economic development which is bound to lead to rapid rates of industrial expansion at the expense of agriculture; i.e., both agricultural productivity and rural standards of living.

The extent to which Chinese Communist planners adopted this model as their own may be illustrated by a comparison of the rate and structure of investment during the first Soviet and Chinese Five Year Plan periods as shown in Table 1. These data must naturally be interpreted with considerable caution. First of all, the national income and investment estimates are necessarily more tentative for China than for Russia. Second, the sectoral definitions are not quite the same in the two cases. Moreover, the data for the interindustry allocation of investment refer only to investments by the state. To the extent that the

[1] Alexander Erlich, "Stalin's Views on Soviet Economic Development" in *Continuity and Change in Russian and Soviet Thought,* ed., E. J. Simmons (Cambridge, Mass., 1955), pp. 81-99.

importance of nonstate investments is not the same in the different sectors of the two countries, Table 1 could present a misleading picture.

Subject to these qualifications, the data in Table 1 suggest that in some respects the Stalinist features were even more pronounced in the Chinese than in the Soviet case. While rates of investment were lower in China, the difference between the two does not appear to be very great. Actually, considering how low Chinese per capita product is relative to the Soviet level of 1928, it is surprising indeed that such a high rate of saving could have been attained. However, the Stalinist tendencies appear most clearly in the investment allocation pattern as evidenced by the fact that the Chinese channeled a significantly larger share of investment resources into industry and most particularly into heavy industry. At the same time, both at the planning and at the realization stage, Chinese Communist policy-makers paid correspondingly less attention than their Soviet counterparts to agriculture and to social overhead.

TABLE 1

RATES AND PATTERNS OF INVESTMENT IN THE SOVIET UNION
(1928–32) AND IN MAINLAND CHINA (1953–57)

	China		Soviet Union	
	1953	1957	1928	1937*
Rate of investment......	16.0%†|	20.0%†	21–23%‡	19–23%‡
Pattern of Investment	PLANNED		REALIZED	
	China	Soviet Union	China	Soviet Union
Sector				
Industry	61.8%	40.7%	56.0%	49.0%
Of which heavy industry...........		31.3%	49.0%	42.1%
Agriculture..........	6.2	15.3	8.2	19.1
Transport............	17.1	21.1	18.7	17.6
Communications......	7.2	0.6		1.1
Trade and procurement				1.8
Social-cultural services and administration..	7.7	22.3	17.1	11.0

* There are no reliable national income estimates for 1932; therefore 1937 as the end year of the Second Soviet Five Year Plan was taken as illustrative.
† Gross investment as a ratio of gross national product.
‡ The lower rates represent gross investment as a ratio of gross national product at established market prices, while the higher rates are in terms of adjusted factor costs.
SOURCES: For China: A. Eckstein, *The National Income of Communist China* (Glencoe, Ill., 1961), Table 15; W. W. Hollister, *China's Gross National Product and Social Accounts, 1950–57* (Glencoe, Ill., 1958); C. M. Li, *Economic Development of Communist China* (Berkeley, 1959), Table 1, p. 9; State Statistical Bureau, *Communique on Fulfillment and Overfulfillment of China's First Five-Year Plan* (Peking, Apr. 13, 1959). For the Soviet Union: A. Bergson, *Soviet National Income and Product in 1937* (New York, 1953), Table 8, p. 75 and Appendix Table 3, pp. 136–37; O. Hoeffding, *Soviet National Income and Product in 1928* (New York, 1954), Table 6, p. 46; N. M. Kaplan, *Capital Investment in the Soviet Union, 1924–51* (Santa Monica, RM735, 28 Nov., 1951), Appendix Table 1, p. 80.

II

The essentially similar approaches to industrialization which characterized the initial five-year plans of Mainland China and Russia produced a rather different outcome in the two settings. As shown in Table 2, industrial production expanded very rapidly in both countries, but while farm output grew—however modestly—in China, it declined markedly in Russia under the impact of forced collectivization.

TABLE 2

CHINESE AND SOVIET RATES OF GROWTH IN INDUSTRY, AGRICULTURE, AND POPULATION DURING THEIR RESPECTIVE FIRST FIVE YEAR PLAN PERIODS

	China		Soviet Union	
1. Average annual rate of growth in industrial production: Official				
Industry..................	16.5%		19.0%	
Industry and handicrafts...	15.0			
Hodgman, large-scale industry.			15.0	
2. Average annual rate of growth in farm production:				
Official....................	4.5		− 4.0	
Johnson...................			− 5.0	
3. Average annual rate of population growth:				
Total......................	2.2		1.3	
Urban.....................	5.0		6.0*	
Rural.....................	1.8		− 0.4*	
4. Cultivated land per capita (in hectares):	1953	1956	1926-28	1938-39
Total population............	.187	.180	.738	.720
Rural population............	.216	.209	.936	1.195

* These are average rates for the 1926–39 period as a whole since urban-rural breakdowns are available only for 1926 and 1939.

SOURCES: For the Soviet Union: D. R. Hodgman, *Soviet Industrial Production, 1928–1951* (Cambridge, Mass., 1954), Table 1, p. 2 and Table 15, p. 89; Harry Schwartz, *Russia's Soviet Economy* (N. Y., 1954), Table 8, p. 127; D. G. Johnson and Arcadius Kahan, "Soviet Agriculture: Structure and Growth," in *Comparisons of the U.S. and Soviet Economies*, Joint Eco. Comm., 86th Cong., 1st Sess., Part I, pp. 204–05; Frank Lorimer, *The Population of the Soviet Union, History and Prospects* (Geneva, 1946): Table 26, p. 67; Table 43, p. 110; Table 44, p. 113; p. 150 and Table 64, p. 159. For China: Helen Yin and Y. C. Yin, *Economic Statistics of Mainland China, 1949–57* (Cambridge, Mass., 1960), pp. 4, 25, 26, 42, and 43.

As indicated in Table 2, for the most part these are official data and as such they are subject to an upward bias. However, the fact that the degree of bias cannot be assumed to be the same for China and the Soviet Union impairs the comparability of the figures for the two countries. The availability of a number of studies such as those of Hodgman and Johnson enable us to correct the bias for the Soviet Union. Un-

fortunately, such adjustments are as yet not possible for China due to the absence of independently derived indices of industrial and agricultural production.

Professor Li's paper clearly demonstrates the inadequacy of Chinese agricultural statistics in particular. There is no question that the apparent rise in farm output between 1953 and 1957 is at least in part statistical rather than real, largely due to improvements in the system of crop reporting and concomitant expansion in statistical coverage. Nevertheless, all of the available evidence suggests that there was some rise in farm production, although less marked than shown by the figures in Table 2.

If this is indeed the case, how can the differing trends in Soviet and Chinese agricultural production be explained? The answer to this question must be sought in the less violent course of agrarian transformation in China. Thus, while collectivization proceeded just about as swiftly as in the Soviet Union—being consummated between 1953 and 1956—it did not provoke the peasantry to active resistance. Therefore it did not, as in the Soviet Union, lead to drastic disruption of farm organization with consequent declines in agricultural production accompanied by mass slaughter of livestock.[2]

III

The different course of collectivization and agricultural production in China as compared to Russia had a far-reaching impact upon all sectors of the economy; i.e., the rate of involuntary farm saving, trends in industrial real wages and labor productivity, the pattern of rural-urban migration, and the structure of investment.

The drastic curtailment of Soviet farm output under the impact of collectivization, just at a time when agriculture was called upon to supply an expanded industrial labor force and to provide in addition an export surplus for financing capital goods imports, meant that a growing share of a shrinking agricultural product had to be saved. This in turn produced a vicious circle: the more farm output declined, the more the rate of involuntary saving had to be raised in agriculture. However, increases in the rate of involuntary saving only served to reinforce peasant disincentives, thereby contributing to further reductions in agricultural production.

In spite of increasing extractions forced out of agriculture, the Soviets encountered serious difficulty in provisioning the cities. This, in

[2] For an analysis of some of the reasons that produced the divergent patterns of collectivisation in China and Russia, see the author's "Manpower and Industrialisation in Communist China, 1952-1957" in *Population Trends in Eastern Europe, the USSR and Mainland China* (Milbank Mem. Fund, 1960), pp. 158-60.

turn, set up another vicious circle. Urban shortages of foodstuffs and other consumer goods led to a drop in real wages with consequent disincentive effects reflected in declines of labor productivity. Therefore, if rates of industrial growth were to be raised or even maintained, more workers had to be imported from the countryside. However, growth of the urban labor force depressed industrial labor productivity only further, both because it aggravated the supply problems and thus led to another round of real wage decreases and also because it brought more and more unskilled labor into industry.

In China, we observe a rather different pattern. With no comparable curtailment in agricultural production, there was no marked disruption in food supply and distribution to the cities. At the same time, as imperfect as the evidence is, it strongly suggests a rising trend in real wages and industrial labor productivity. As a result, greater reliance could be placed upon increases in labor productivity as a means of obtaining high rates of industrial growth. Moreover, there was considerable room for expanding the industrial labor force just by stepping up the rate of urban labor utilization, first by absorbing the unemployed and then by increasing the labor participation rate. Due to all of these factors combined, a pace of industrial expansion that was not too far behind that attained in the Soviet Union could be achieved with relatively slower rates of urbanization.

Given the differential natural rates of increase in the populations of Russia and China as shown in Table 2, rates of rural-urban migration must have been significantly lower in China. This trend is also reflected in the fact that while rural population declined slightly in the Soviet Union, it continued to grow quite rapidly throughout the first five-year plan period in China.

These differences in the course of collectivization and urbanization account, at least in part, for the lower investment share allotted by the Chinese to agriculture and to social overhead as noted earlier in Table 1. Unlike the Soviets, they did not have to mechanize agriculture just to replace slaughtered animal draft power. At the same time, with a slower pace of urbanization in China, pressure on public utilities and municipal facilities was not as acute as in the Soviet Union. Another consequence of the slower rural exodus was the increasing damming of population in the Chinese countryside as evidenced by a decline in cultivated land per capita between 1953 and 1957.

Therefore, Chinese Communist policy-makers by carefully avoiding the Soviet collectivization debacle in effect faced a new set of dilemmas in the field of population control. Apart from doctrinaire incantations against Malthusianism, up to 1955 the Chinese Communist leadership apparently paid little attention to the population problem. However,

rising rates of natural increase, primarily due to a reduction in mortality rates, forced a re-evaluation in population policy. As a result, some birth control measures were instituted between 1955 and 1957.[3] Yet, this new population policy was only half-heartedly pursued, since the leadership could not make up its mind whether to follow its doctrinaire bias and treat population as a productive resource, as a source of labor supply, or whether to stress its role as an actual and potential impediment to increasing saving and investment.

IV

In spite of five to eight years of rapid industrial growth accompanied by relatively nonviolent collectivization, Chinese Communist policy-makers approached the end of their first Five Year Plan with some serious unresolved problems on their hands. Within this context, they began to grope for a new development strategy, one that would provide a way out of the dilemmas facing them. The most intractable issue confronting them was agricultural stagnation. Farm production grew only slowly, possibly just sufficiently to keep pace with population growth. Unless this trend could be reversed, agriculture would increasingly retard the pace of industrialization in a more or less closed economy. Therefore, Chinese Communist planners were seeking a strategy which would promote growth in farm production without significant diversion of investment funds from industry to agriculture.

The problem was aggravated by the rising rate of population growth and the increasing pressure of population on arable land resources as shown earlier. The frantic search for an escape from the "low-level equilibrium trap" was thereby only accelerated.[4] All of these problems converged in the course of 1957, when the pressure on domestic saving was also rising, due to the approaching exhaustion of Soviet credits to China.

The essence of the problem facing China's planners was most succinctly defined by Eckaus in the following terms:

Suppose that the respective demands for output are such that a large part of the available capital is drawn into the capital-intensive and fixed coefficient sector. The amount of labor which can be absorbed in these sectors is dependent on the amount of capital available. Since capital is a scarce factor, labor employment opportunities in this sector are limited by its availability rather than by demand for output. The relatively plentiful labor supply is then pushed into the variable-coefficient sector and absorbed there as long as the marginal value productivity of labor is higher than the wages it receives.[5]

[3] For a detailed analysis of Chinese Communist population policy, see Irene Taeuber, "Population Policies in Communist China," *Population Index*, Oct., 1956, and W. Parker Mauldin, "Fertility Control in Communist Countries," in *Population Trends in Eastern Europe, The USSR and Mainland China* (Milbank Mem. Fund, 1960), pp. 197-208.

[4] R. R. Nelson, "A Theory of the Low-Level Equilibrium Trap," *A.E.R.*, Dec., 1956.

[5] R. S. Eckaus, "Factor Proportions in Underdeveloped Areas," *A.E.R.*, Sept., 1955, pp. 559-60.

It is against this background that a new development strategy began to crystallize in 1958—one better suited to China's factor endowments on the one hand and her planners' scale of preferences on the other hand. At its core, this strategy involves mass mobilization of underemployed rural labor on a scale not attempted before, even in China.

This additional labor is to be largely used locally for three purposes: (1) labor intensive investment projects such as irrigation and water reclamation, (2) more intensive methods of agricultural production based on greater applications of labor designed to increase unit yields through closer planting, more careful weeding, etc., and (3) development of small-scale industry. Moreover, all of this is to be accomplished by preventing leakages into consumption, thus capturing all of the increase in marginal product at zero marginal cost. In effect, then, this represents an application of the Nurkse model of capital formation in its purest form.[6]

Of course, none of these were entirely new measures. Mass labor projects are based on an ancient tradition in China and have only been perfected and rationalized by the new Communist regime. However, rural labor mobilization prior to 1958 was much less comprehensive and systematic than since.

One of the interesting by-products of this new strategy was a shift in Chinese population policy. As was noted above, between 1955 and 1957 there was considerable—although somewhat equivocating—concern about the rapid rate of population growth and its implications for economic growth.[7] With the new emphasis on labor as a productive resource, population again was viewed as an asset rather than a liability. This is most clearly illustrated by the following quote from Liu Shao-ch'i:

All they see is that men are consumers and that the greater the population, the bigger the consumption. They fail to see that men are first of all producers and when there is a large population there is also the possibility of greater production and accumulation.[8]

It was already indicated that the development of small-scale industry was one of the uses to which the rural underemployed were to be put. While small-scale industry has been traditionally a subsidiary occupation for the Chinese farm population, it was mostly confined to weaving of textile cloth and other handicrafts. Within the context of the new strategy, Chinese Communist planners view it as one of the principal means for increasing the rate of industrial growth. In effect, they con-

[6] For an official statement of policy, spelling out the new strategy, see Liu Shao-ch'i's *Report on the Work of the Central Committee*, delivered at the Second Session of the Eighth Congress of the Chinese Communist Party on May 5, 1958.
[7] See for instance, Ma Yin-ch'u's "A New Principle of Population," *Jen-min jih-pao* (Peking, July 5, 1957).
[8] *Ibid.*

centrate on the simultaneous development of two distinct industrial sectors: a modern, large-scale capital-intensive sector based on fixed factor proportions and a small-scale labor-intensive sector based on variable factor proportions. In pursuit of this policy of technological dualism, or "walking on two legs" as it is officially termed in Chinese Communist writings and pronouncements, the expansion of small-scale industry is promoted in a number of sectors such as iron and steel, machine shops, fertilizer production, power generation, coal extraction, in addition to the more traditional textile and food processing industries.[9]

The strategy of dualism is, however, not confined to its purely technological and factor proportions aspect. On the contrary, it seems that the model is extended to incorporate the notion of rapid development of a national economy, but based on two almost separate economies within it, only loosely linked through interregional and rural-urban trade. According to this concept, the state would concentrate the preponderant bulk of its investment resources on the development of the modern sector. This is a sector with a high reinvestment quotient, with practically all of this reinvestment to be channeled into continuing growth of itself. At the same time, the diversion of output from the modern to the rural sector is to be minimized. Therefore, the expansion of the rural sector should be a function of its own output and investment.

Small-scale industry is to be developed by using simple equipment manufactured locally, local labor, and local raw materials. The output of these industries would then be used to satisfy the rural demand for manufactured consumer goods and agricultural requirements for production requisites. The rural sector is thus pushed into involuntary and partial autarky—partial, in the sense that while the rural sector should not import from the modern sector, it would be expected to provide a large unrequited export surplus to it. Thus the rural sector would need to save enough of its current income to finance its own development while contributing to the growth of the modern sector.

V

How literally and with what results has this new strategy been implemented in Communist China? Attempts to apply it in its purest form were most pronounced in 1958—particularly in the second half of that year. As the strategy evolved, the policy-makers were clearly groping for an institutional instrument suited to mass mobilization of rural

[9] This aspect of the new strategy was officially enunciated in the Communique of the Sixth Plenary Session of the Eighth Central Committee of the Chinese Communist Party issued on Dec. 17, 1958.

labor along lines outlined above, since the existing institutional framework of agriculture was not adapted to an effective implementation of the model.

By the end of 1956, practically all of Chinese agriculture was encompassed by small collectives (officially termed "producers' co-operatives of the advanced type") of 35 to 100 households each. Management, supervision, and control of such a vast number of small units placed a considerable strain upon the administrative and party apparatus. Moreover, their proliferation and small size made them ill-suited as units of mass labor mobilization and utilization. Therefore, during 1958 a number of such small collectives were merged to form communes. These new units were sufficiently large to harness major labor-intensive projects beyond the resources of the collectives and integrate agricultural production with the mass labor projects on the one hand and the development of small-scale industry on the other. At the same time, the communes served not only as an instrument for the better utilization of the existing labor force, but also for augmenting the labor force with women released from housework. Last, but not least, the task of managing consumption controls and preventing leakages must have appeared easier with a smaller number of large units.

In its first year (i.e., 1958), the application of the new strategy, coupled with the organization of communes, was characterized by improvisation, lack of realism, misstarts, and a great deal of waste. This was perhaps most pronounced in the mass movement to produce iron and steel in the backyard. As is well known by now, the quality of the resulting product was so defective that much of it had to be scrapped. Yet it would be erroneous to base one's judgment of the success or failure of the strategy as a whole on this single example.

First of all, there are definite indications that the mass applications of labor in 1958 led to a considerable extension of the irrigated area and to significant increases in unit yields resulting therefrom. While there is no question that in this field particularly the official claims are exaggerated, I suspect that when careful studies of China's agricultural production now in process are completed, they will show a discreet leap in agricultural production in 1958, although much below the official claim.

Second, in respect to small-scale industry growth, Chinese Communist planners seem to have learned from their failures in 1958. Realizing that they overreached themselves, they continue to push vigorously for the development of these industries but on a more modest and rational basis. In the course of 1959 and 1960, considerations of technical feasibility received more attention. At the same time, it was recognized that the rural sector could not be thrown back on

just its own resources; if it was to grow, it had to receive technical assistance as well as some investment goods from the modern sector. Thus in the course of its adaptation to reality, this model—like all others—lost some of its purity.

Therefore, in the course of implementation, the new strategy undoubtedly yielded a lower rate of economic growth than would have been theoretically possible. But the critical question is whether it succeeded in raising the rate of growth during the current Five Year Plan period as compared to the preceding one. This question cannot be answered categorically at the present time. Most disturbing from our point of view, the quality of Chinese Mainland statistics deteriorated very considerably in 1958 and 1959, which renders an appraisal that much more difficult. Even so, when studies near completion or under way, such as those by Professor Liu, Li, and others, are published, we will undoubtedly have a firmer basis for arriving at a judgment. In the meantime, the evidence that we already have would support the tentative conclusion that the new strategy did indeed succeed in raising China's economic growth during 1958-59 above the average levels attained in the preceding years.

DISCUSSION

FRANKLYN D. HOLZMAN: Professor Eckstein, in his interesting paper, has argued that the Chinese followed in Soviet footsteps of concentrating on capital-intensive industrial projects during their First Five Year Plan. During the Second Five Year Plan, however, they opened up a second front of labor-intensive investments in both industry and agriculture, thereby embracing "technological dualism." I find his arguments for this hypothesis convincing and accept most of the implications he draws from this development. I will confine my comments to three points of clarification and amplification concerning: (1) Eckstein's underestimation of the Soviet rate of investment; (2) the extent of "technological dualism" in underdeveloped areas; and (3) the reason for disparities between China and the U.S.S.R. in labor productivity growth rates.

1. Professor Eckstein supports his argument that the Chinese planners followed the Soviet model by indicating in his Table 1 that the structure and rates of investment in the two countries were very similar. He compares Chinese rates of investment of 16 and 20 per cent in 1953 and 1957 with Soviet rates of 21-23 and 19-23 per cent in 1928 and 1937, respectively. These are roughly similar rates. Since China in 1953-57 was a much poorer and less-developed economy than the Soviet Union in 1928-37, Eckstein is surprised that it was possible for the Chinese to extract (relative to the Soviets) such a high level of savings from their population.

Unfortunately, 1928 and 1937 are the only years for which estimates of Soviet investment rates have been generally available. It is my opinion, however, that the rates of investment experienced in most or all of the intervening years were substantially higher, probably reaching 40 per cent in some years. This statement is made on the basis of the following evidence. Francis Seton [7] has compiled social accounts for the Soviet economy for the year 1934. He estimates a rate of investment of 26.5 per cent in the very distorted Soviet market prices. Making crude adjustments of his figures for sales taxes and subsidies to get both gross investment and GNP at factor cost yields a rate of investment of at least 35 per cent. It seems beyond question that the rate of investment was even higher in the years 1930-32 because levels of consumption were relatively so depressed.[1]

Absence of careful studies, with the exception of Seton's little-known work, for the period 1929-36 has led Professor Eckstein and many other scholars to underestimate substantially the intensity of the Soviet industrialization drive in the first two five-year plans. Thus, Soviet rates of investment, instead of being just slightly higher than Chinese rates over comparable planning periods, may have been from 50 to 100 per cent higher; and the forced sav-

[1] Military, and therefore government, expenditures were also very low from 1928 through 1935, rising sharply in 1936 and 1937. This would have helped depress the rate of investment in 1937 in comparison with earlier years.

ings extracted from the Russian people appear more comparable to those extracted from their Chinese counterparts.[2]

2. While I have no doubt that the scale on which the Chinese are currently engaging in "technological dualism" surpasses anything ever attempted by the Soviets, I think it is worth pointing out that a much less spectacular variety is constantly practiced by the Soviets, and probably by the Chinese and most other underdeveloped nations, and right within heavy industry itself. Most persons who have visited large Soviet industrial plants have been struck to find, under the same roof, highly mechanized operations on the one hand and extremely labor-intensive operations on the other. I personally visited a large turbine factory in Kharkov and was alternately awed by the overhead cranes, giant lifts, and other highly mechanized equipment, and bemused by the numerous unskilled workers sweeping floors with brooms made of tree branches, pushing old and clumsy carts, etc. The extent of this "dualism" is crudely quantified in comparative statistics which show the Soviets with an industrial labor productivity of less than half our own. Some specific examples: a large Soviet blast furnace at Kuznetsk was manned by 195 workers; a similar furnace in the United States required 95. Another example: in 1936, Inland Steel required 260 men to operate 7 open-hearth furnaces whereas the Soviet Makeyevka plant employed 1,070 wage earners to operate 6 similar furnaces [2, pages 130-31]. These differences between U.S. and Soviet plants are largely due to the sweepers, cart-pushers, and other unskilled workers found in Soviet factories but whose jobs have been mechanized in the United States. This sort of dualism makes a lot of economic sense in a situation where labor, especially unskilled labor, is plentiful and capital is scarce. Subdivide the production processes within a plant into those which give large returns to scale and a high rate of return on capital and into those which do not; then mechanize only the former. The presence of technological dualism right within single industrial plants substantially reduces, in my opinion, the applicability of analyses of underdeveloped areas such as cited by Eckstein which depend upon the existence of fixed coefficients in the capital-intensive sector.

3. Professor Eckstein blames the drop in Soviet industrial labor productivity in the early thirties on the drop in agricultural output in the Soviet Union following collectivization on the basis of the following set of interactions: drop in agricultural output, drop in real wages, decline in labor incentives, drop in labor productivity and industrial output, increasing industrial employment to offset declining productivity,[3] still further drop in real wages, output, and productivity, etc. While I agree that the decline in industrial

[2] Eckstein admits in his paper that Soviet real wages declined over the First Five Year Plan whereas Chinese real wages rose (see blow). Absolute levels of real wages were, of course, much lower in China.

[3] I think the major reason for the larger-than-planned increase in industrial labor force was not declining productivity but the fact that the Soviet plans were unrealistically optimistic in this period and targets were set for industries and plants which were unattainable in terms of raw material and equipment availabilities. The only factor of production in plentiful supply was unskilled labor; plant managers, in attempting to fulfill their plans, substituted unskilled labor for other inputs wherever this was feasible.

labor productivity may have been encouraged by the fall in real wages, I do not feel that the latter was the crucial independent variable. First, the decline in real wages, while clearly substantial and apparent to workers, was in part disguised by a money illusion; viz., money wages were rising by 20-25 per cent a year all the time that prices and price-increasing taxes were rising (at a more rapid rate, of course) [3]. Second, work incentives are a function not only of the absolute level of wages but of the structure of wages; and both the highly differentiated wage structure and the progressive piece-rate system of payment used by the Soviets were calculated to encourage a good allocation of labor and intensive effort on the job, and, so far as I know, were successful along these lines. Third, I am not at all sure that the income effects of declining real wages did not have a more powerful impact on work incentives than the substitution effects. This is attested to by the facts that many workers held more than one job and that the number of employed persons per family increased in the early thirties.

Why, then, did Soviet labor productivity decline in this period? Four reasons come to mind: (1) a labor turnover in industry (related to the inflation) which exceeded 100 per cent; (2) an approximate doubling of the size of the nonagricultural labor force in a five-year period with peasants who had never seen the "broad side of a piece of machinery" and who required long periods of training to become efficient workers; (3) the generally poor quality of Soviet planning in this early period which was as much characterized as not by transport breakdowns, commodity bottlenecks, misallocations, and other difficulties; and (4), to be discussed below, the fact that the non-agricultural labor force increased more rapidly than did capital stock in industry. When these factors were partly or wholly overcome during the Second Five Year Plan, labor productivity rose rapidly. It seems highly unlikely that the very small increase in real wages in this period could have been responsible for this productivity rise.

Why did Chinese industrial productivity not decline over the First Five Year Plan? Unfortunately, I cannot answer this question with any real assurance. My hunch is that all of the factors just mentioned which slowed up Soviet growth were lesser deterrents in the Chinese case, the Chinese planners having had the benefit of Soviet experiences. In particular, I think the differences between the Chinese and Soviet rates of productivity growth were largely due to the differences in incremental capital-labor ratios over their first five-year plans, and I have indulged in a bit of very casual empiricism to demonstrate this point. From 1928 to 1932, the Soviet nonagricultural labor force increased by 110 per cent and nonagricultural capital stock by 60 per cent for an incremental capital-labor ratio of about 0.55.[4] With labor force increasing almost twice as rapidly as capital stock, one might even have expected a decline in Soviet labor productivity, all other things equal.[5] The

[4] These figures are estimated from [5, p. 52.]. The well-known methodological deficiencies in Soviet capital stock data could hardly be serious enough to affect the conclusions presented below.

[5] For a final judgment on this matter, one would, of course, have to take account of the very important factor of changes in quality and composition of capital stock.

incremental capital-labor ratio for China is crudely estimated as having been three to six times as great, ranging from about 1.8 to about 3.3. In the Chinese First Five Year Plan, capital stock increased by roughly 35 to 65 per cent whereas the industrial labor force by only about 20 per cent. Thus, in China, the average worker had more capital to work with at the end of the first plan whereas in the Soviet Union, the average worker had less.[6] Finally, it should be pointed out that during the Soviet Second Five Year Plan, when Soviet industrial labor productivity increased so rapidly, the labor force increased by only 20 per cent and capital stock by 90 per cent [5, page 52] for a capital-labor ratio of 4.5, higher than in either of the previous instances.

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SIDNEY KLEIN: I should like to begin with the obvious. Professor Li's paper is a major contribution to the literature on Communist China. It is a study which is a short-run contribution in that it sheds light on specific economic developments in China, 1949 to present, and is a long-run contribution in that Professor Li reminds us rather pointedly that there is nothing so basic or central to a discipline—in this case economics—as its principles. Let us consider the broader aspects of his paper first. In the past eleven years, the Chinese Communist Party has made massive and perhaps historically unprecedented efforts to transform a large, sprawling, primarily agricultural technologically primitive nation into a modern industrial Goliath. With respect to agriculture, industry, transportation, communications, money, banking, prices, employment, trade, etc., year in and year out, it has reported what has seemed to be incredible progress. In terms of both absolute numbers and index numbers, the improvements in all fields the Chinese Communists registered for

[6] These Chinese estimates are crude in the extreme and were thrown together in the hope and belief that they indicate a correct order of magnitude. They are borne out by Table 9 in Choh-Ming Li's book [4, p. 235] which estimates that fixed capital assets per worker in industry increased from 5,656 yuan in 1952 to 6,835 yuan in 1956. Our estimates of the capital-labor ratio were made from the following data. Labor force increased from 12.2 million in 1952 to 14.3 million in 1956 [4, p. 233]. The assumption was made that the same rate of increase occurred in 1957. The 1952 capital stock was estimated by getting nonagricultural (industrial) output figures for 1952 and applying to these figures a capital-output ratio of 3. Nonagricultural output was estimated from Eckstein as having amounted in 1952 to roughly 20 million yuan [1, p. 134]. An alternative estimate—of gross industrial output—of 34 million yuan taken from Li [4, Table 4]. Applying our capital-output ratio of 3 to these figures yields a range for capital stock of 60 to 112 million yuan. The increment to this capital stock was assumed to be current nonagricultural investment over the Five Year Plan taken from [6, p. 42].

themselves made them the envy of underdeveloped nations and the worry of non-Communist others. Inevitably, some of the underdeveloped countries either contrasted or found individual economists contrasting unfavorably their own apparently feeble efforts with the widely publicized giant strides of the Chinese. India is clearly the best case in point. In 1956, an Indian delegation visited China specifically to study agricultural planning and techniques. Sporadically in 1957 and 1958 and then in snowball fashion in 1959 and 1960 came articles in leading professional journals in India, the United States, and other places laudatory of China's economic accomplishments and minimizing those of India. Whether the articles' authors did or did not make the point explicitly, some of their readers received the impression that in a general kind of way there appeared to be a close relationship between the disparate political and social *modus operandi* of China and India and the disparate performances of their economies. Occasionally, a dissenting voice would point to the environments for statistical purposes prevailing in China and India and attempt to invoke the doctrine of *caveat emptor*, but he was usually overpowered in the rush to apply advanced tools of economic and statistical analysis to the voluminous data being issued from Peking and New Delhi; and by the rush of editors to publish such studies.

Now, with Professor Li's study available, there cannot be any question about the fact that the validity of such articles has rested, and for some time will continue to rest, on sandy, easily dissipated statistical bases. Unlike the past, it is not likely that henceforth economists will use with unrestrained confidence data which were subject to personal and political manipulation at the point of origin and to political manipulation at all levels above the point of origin; to data which reflect weighing, measuring, and counting activities which were not checked for adulteration or other fraud; to data which ignore differences in scope, depth, definitions, and computation methods in the schedules issued by the planning, statistical, and political authorities; and to data produced by individuals an overwhelming majority of whom had never had any special training in statistics, earned low wages, had low morale and high employment turnover rates, and who repeatedly lost, arbitrarily altered, and deliberately fabricated statistical data. In short, it is likely that in the future more attention will be paid to elementary statistical matters.

Turning to more specific issues, I should like to call attention to Professor Li's findings concerning the relative reliability of the different types of statistical data available. It appears significant that the least unreliable data emanating from Mainland China from 1949 to present have dealt with state-owned, centrally-controlled industrial enterprises and have related to physical output and value product—items which can be relatively readily computed. Similarly, it is significant that the most unreliable statistics have dealt with the finances of individual farm families and are based on records of original entry which are most difficult for the state to verify. Aside from the obvious conclusions concerning the efficacy of government controls and widespread, preferably universal, literacy with respect to statistical data collection efforts, it seems to me that at least one other conclusion may be drawn. It would appear that the peasants of China are doing under the Communists what

they did under the Nationalist, war lord, Manchu, etc., governments before them: hiding food and other valuables from the authorities to prevent their confiscation. In recent years, as in earlier periods, where the needs of the farm families and the government have conflicted, the peasants appear to have resolved the income distribution problem in favor of themselves. As a minimum, it would also appear that either their instinct for survival or their acquisitive instinct has not been extinguished in the past eleven years.

Related to this situation is still another specific contribution of Professor Li. He has affirmed with respect to statistical matters the wide powers exercised by the local units of the CCP and their governmental and economic appendages. After 1949, as before it, there were frequently wide differences between the economic and other policies laid down by the central government and the practices pursued in the agricultural communities of China. To use Communist terminology, the local units of the CCP have frequently been guilty of "left deviationism" and "right deviationism." That is, they have frequently gone too far with respect to implementation of the decisions of the Central Committee or they have not gone far enough. The content of Professor Li's methodical study and my own excursions into the literature available lead me to believe that the post-1949 deviations have been and still are as relatively numerous quantitatively and as relatively wide in amplitude qualitatively as the better-known ones which took place in Soviet China, 1931-34, and in the Communist-held areas of Free China during the Sino-Japanese War. This explains, in part, the differing versions of life in the villages of China from 1949 to the present, offered by the CCP on one hand and refugees streaming into Hong Kong on the other.

In closing, it seems appropriate to note that in China as in the Western world, one has to contend with lies, big lies, and statistics.

JOSEPH S. BERLINER: There are two kinds of virtue a critic might claim for the evaluation of a paper such as that of Drs. Liu and Yeh: sophistication in the subject or innocence in the subject. The special virtue of sophistication is the capacity to answer questions. If there is a special virtue to innocence, it is, I suppose, the capacity to question answers. My remarks will be heavy on questions.

The national income estimates presented here are built up from Chinese published data on gross value of output by sector of origin. The principal adjustments of the official data are made in food crop production and industrial production. The appropriateness of these adjustments is particularly significant because agriculture accounts for about 40 per cent of national income, and industrial production, while accounting for a smaller proportion of the total, is however the fastest growing sector.

The reason for the rejection of the official food crop data for 1949-55 is based on a comparison of those data with calculated per capita calorie intake figures. Since the calorie intake figures imply a contrary-to-fact condition of general starvation, the production data are deemed to be faulty. I should like to entertain for a moment the hypothesis that the production data are accurate but that the calorie intake figures are faulty. The hypothesis is worth

considering because of the rather considerable number of steps in the process of conversion of total production to per capita calorie intake, each step requiring a new set of assumptions of fact. The steps include adjustments for the conversion of total output to output for human food consumption, for exports and imports, for processing losses in both farm-processed and mill-processed foods, transportation and spoilage losses, food sources of field troops during the Korean war, and, finally, that highly variable magnitude, population.

Per capita calorie intake is a useful indicator when the data are based on a direct sample survey of food consumption in households. But if the data are the end product of an extensive chain of tenuous estimates, their significance is difficult to evaluate. In fact, the authors' case rests on a considerable body of evidence, independent of the calorie-intake calculations; the only question I raise is whether the case is strengthened by those calculations.

Agreeing, then, that agricultural output for the years before 1956 is probably understated in the official data, how ought the data be adjusted? Our authors argue that the 1956-57 output data imply consumption levels so low that the 1952-55 consumption levels cannot reasonably be thought to have been any lower. Output for those earlier years is then estimated simply by multiplying an assumed constant per capita food consumption rate by population. The effect of this procedure is to assume away virtually all growth in a sector that accounts for well over a third of the national income. In evaluating the over-all rate of growth it is therefore important to keep in mind that a substantial element of growth in the official data has been eliminated by assumption.

The source of the authors' doubts about the official industrial production data is the fact that the sum of the value of output of individual commodities for which data are available is rather substantially less than the reported aggregate value of all industrial production. In the case of producer goods, the portion of the total unaccounted for is fairly stable over time, but in the case of the consumer goods, the portion unaccounted for increases over time. One might handle this puzzling gap in three ways. First, the official aggregates could be accepted as reported, on the argument that we cannot make any reasonable statement about the value of output of products for which no data are reported. This decision amounts to saying that whether or not we trust the Communist data in the absence of independent checks, we have no choice but to accept them. At the other extreme, we could enter into the accounts only those products for which data are available, throwing out the value of output not accounted for by detailed product data. This decision amounts to saying that anything the Chinese may have produced but not reported will be treated as if it had not been produced. Our authors' decision is a reasonable compromise between these extremes. They are willing to accept the Chinese government's word that they are producing some things not reported by product, but only to the extent that the unreported output does not grow more rapidly than the reported. Accordingly, the official aggregate producer goods data are accepted as reported, but the aggregate consumer goods data are scaled down to make the rate of growth of the unreported items equal to

that of the reported. The method is eminently reasonable, but it is worth pointing out its essential arbitrariness, since the decision of the Chinese regime to report output figures of certain commodities rather than others is taken on political rather than on economic grounds.

One final comment on the rate of growth of national income. People accustomed to think in terms of Soviet statistics tend automatically to ask a national income analyst to declare himself: Are you for Paasche or Laspeyres? Our authors present in fact three estimates of the growth rate, using 1933, 1952, and 1957 price weights, corresponding approximately to preindustrialization, midyear, and postindustrialization weights. The average annual growth rates are 4.4 per cent, 6 per cent, and 5.7 per cent. Here is a puzzle, for the "index number effect" would lead us to expect the rates to be lower when weighted by later-year prices. The solution to the puzzle is that, while 1933 prices were established on a relatively free market and approximated factor costs, the 1952 prices reflect the government's policy of depressing agricultural prices relative to industrial. Thus relative market prices moved in a direction opposite to the direction that relative factor costs presumably moved. If this is so, then a recalculation of national income at factor costs should show growth rates of less than 4.4 per cent per annum if 1952 or 1957 factor-cost weights were used. Such low growth rates would be surprising indeed, and might cause our authors to reconsider the magnitude of the downward adjustments made in the official agricultural and industrial data.

TA-CHUNG LIU and K. C. YEH: The first question raised by Professor Berliner bears upon a point which is perhaps the least controversial among all questions concerning the Chinese Communist economy: the lack of rapid and substantial progress in agriculture. Our calculation of the postwar calorie intake levels do not involve as many assumptions as Professor Berliner mentioned; for the principal data used in this calculation, i.e., the proportion of food crops used for food purposes domestically (which proportion has taken into consideration such factors as exports and nonfood uses), are Communist own data. While population data are involved in the computation of the 1933 and the 1957 levels of per capita calorie intake, the rock-bottom level implied in the average ration allowed in 1957 is computed from figures given directly on a per head basis in the Communist rationing schedules. Moreover, the "backward projection" from 1957 to 1952 involves only the rates of change of population, not the absolute size of population as such. Our estimate of the 1933 level of calorie intake does involve roughly the assumptions mentioned by Professor Berliner; but it is precisely the 1933 estimate that has the support of direct sample consumption surveys which Professor Berliner believes to be more trustworthy.

The assumption of constant per capita consumption of food crops does not mean that total production had not increased. For population was increasing at roughly 2.5 per cent per year, even though total production of food crops increased at a somewhat lower rate during 1952 to 1957 because the proportion of food crops used for food purposes, according to the Communist own data, increased in this period. Considering the facts that formerly unculti-

vated land has been brought under cultivation and that very little chemical fertilizer (on a per acre basis) has been produced, it must not have been easy to increase production of food crops at a rate roughly comparable to that of population.

Our adjustment of the Communist data on manufactured consumers' goods may be considered as the fourth way, in addition to the three enumerated by Berliner. It is to examine the plausibility of the very fast rate of increase of the "unidentified goods" by trying to find out what these goods could have been. For example, we have gone as far as trying to account for cosmetics, to which the Communists apparently did not pay a great deal of attention. While we cannot say that our search is exhaustive, it is in fact difficult to think of any important commodities that have not been included in our enumeration. Our adjustment of the Communist data on consumers' goods (i.e., to assume the rate of increase of the "unidentified" portion equal to that of the "identified" portion) is admittedly arbitrary; if it has erred, it has probably overestimated the rate of increase of consumers' goods.

With regard to the "index number effect," the "solution to the puzzle," mentioned by Berliner, can be found in the paper referred to earlier (see footnote 2 of our paper) and in a larger manuscript to which Berliner has had access. The real question is whether an over-all rate of growth of national income of 4.4 per cent per year (in terms of 1933 prices) or lower (in terms of factor costs in postwar years) is or is not too low. Berliner finds such low rates "surprising." Our position is that there is no a priori basis on which to make a judgment. It is important to realize, however, that this 4.4 per cent over-all growth rate implies an average rate of expansion of the producers' goods manufacturing industries of almost 30 per cent per year during 1952-57. The latter rate remains about the same whether the calculation is done on 1933 or 1952 prices. (The index number problem involved here has to do mainly with agricultural prices relative to industrial.) But is an over-all rate of growth of 4.4 per cent (or 6 per cent in 1952 prices) consistent with a rate of expansion of the producers' goods manufacturing industries of almost 30 per cent? This picture would be not at all puzzling if one looks at the employment data. Employment in the relatively modern sectors (manufacturing factories, mining, utilities, the construction industry, modern transportation and communications) of the economy increased from 5.6 million in 1952 to 9.3 million in 1957, an increase of 3.7 million workers. In terms of national power potential, both the rate of increase from 1952 to 1957 and the absolute size of employment in these sectors in 1957 post a tremendous threat to the neighboring Asian nations. However, even in 1957, employment in these sectors accounted for no more than 3 per cent of the total employed labor force. By a concentrated use of resources, the Communists have brought about rapid expansions in certain sectors of the economy; but these sectors are still a relatively small part of the entire economy.

THE INFLUENCE OF MORAL AND SOCIAL
RESPONSIBILITY ON ECONOMIC BEHAVIOR

THE INFLUENCE OF ETHICAL AND SOCIAL
RESPONSIBILITIES ON ADVERTISING
AND SELLING PRACTICES

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I

Behind the outward symmetry which characterizes much of current economic theory lies a body of assumptions concerning the nature of the market and the ethics of its performers. In our economic models, consumers are singularly rational, sellers are singularly honest, products are singularly well defined, and consumption is singularly static. Fraud and deception are in effect outlawed from the market place. The consumer, possessed of precise and understandable units of measurement, weighs his utilities with unusual discernment. Through his free and informed choice, he steers production, and the market is thereby continually shaped and reshaped by the sum of myriad, tiny nudges as each buyer in each and every transaction exercises his sovereignty.

This exchange process is, in a word, assumed to be surrounded by a moral and legal framework which insures a basis for confidence between buyer and seller.[1] This framework not only implicitly includes standard monetary units which permit firm price quotations but it also assumes explicit benchmarks of quantity and of quality equally available to and mutually understood by buyer and seller. Should monetary units become widely counterfeited or should designations of quantity and quality be rendered meaningless, the exchange process would, of course, be reduced to chaos.

We depend then upon a framework of law and custom to provide that basis for confidence essential to orderly exchange. Lacking such a basis, the market place would become a veritable Tower of Babel and "throw into confusion all the people."

In this neat and tidy view of economic theory, the role of advertising, together with its accompanying panoply of sales and promotional devices, has received remarkably short shrift. Sprawling reality, to be sure, has seldom if ever patterned itself to a glove-fit of theory. Fraud,

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[1] The ethical basis of economic activity is well spelled out in J. M. Clark, *Economic Institutions and Human Welfare* (Knopf, 1957), Chap. 2.

for example, has ever been present in our economy. Butchers have been prone to weigh thumbs. False bottoms have been placed in measures. Shoddy has been blended with wool, and nostrums have ever been sold to cure any ailment from catarrh to cancer. Such practices have typically invited legislative action. And while legal suppressants have inhibited only imperfectly such malpractice, the formal extension of the police power of the state into the market place has contributed to confidence—confidence on the part of buyers and sellers in their exchanges and equal confidence on the part of economists that the institutional framework has been so adjusted as to keep market practice reasonably consistent with market theory. Thus the common-sense view that sin and corruption have always been present in man's imperfect world has been translated into theory as a given unpleasant but predictable constant which we trust will be held in check by social and political forces whenever and wherever it threatens to get too far out of hand.

In the main, advertising has been recognized as a market practice peculiarly susceptible to some of the least admirable of human traits. Since the turn of the century, it has been increasingly subject to one or another attempt to contain its exploding force within the confines of a reasonably moral framework.[2] Repeatedly the trade, itself, has called upon itself for a new dedication to "truth in advertising." Fifty years ago, when the Associated Advertising Clubs of the World met in the United States, they adopted as their own the slogan, "Truth in Advertising." Fifteen years later, they were still at it. At the International Advertising Convention at Wembley (England), a Code of Ethics was adopted to reaffirm the ideal of "Truth in Advertising." During the thirties the cry for "Truth in Advertising" as a reform from within continued to be raised at nearly every advertising trade convention. In 1938 a "Truth in Advertising" section—a prohibition against false advertising—was added to the Federal Trade Commission Act through the Wheeler-Lea Amendment.

So far this simple chronology appears to bolster our assumptions. The ethical and social responsibilities of advertising would appear to have found their counterpart in institutional controls. But in the case of advertising, it has become evident that the prohibitory approach has not been effective. Once again this year the organized advertising trade is calling aloud for a re-rededication to the slogan, "Truth in Advertising." This current industry dedication lacks none of the fervent resolves to reform that attended the many earlier drives. In June, 1960, at the annual convention of the Advertising Federation of America, for example, the publication of the *Advertising Truth Book* was an-

[2] For an interesting study of the history of regulatory controls on advertising, see *The Responsibilities of American Advertising*, by Otis Pease (Yale Univ. Press, 1958).

nounced: fifty-six pages packed with definitions, precedents, criteria, guideposts, rules, standards, test questions, taboos and explanations— all designed, as the trade paper *Advertising Age* put it, "to nail down this abstraction called Truth." And during the coming year, according to the *Wall Street Journal*, this latest "Truth in Advertising" drive is to be dedicated to "The Unsoiled Sale."

Hence the conclusion seems inescapable that whatever influence ethical and social responsibilities may have had on advertising and selling practices in the past, these have been wholly inadequate in the eyes of the advertising industry itself. Pacing—perhaps forerunning—this sense of moral inadequacy within the trade has been a mounting volume of criticism of sales practices in the form of exposé books, Congressional hearings, and complaints to consumer protective agencies.

Although the tide was clearly evident before the television scandals of this past year, the rigged TV quizzes and the extent of payola served to confirm the judgments of the most severe critics. The present situation may, I believe, be fairly evaluated as a crisis of confidence in almost the whole range of marketing practices—a crisis which runs the gamut of goods and services, from the promotion of consumer debt to questionable cigarette claims and from rigged prices to deceptive packaging and planned absolescence. At the moment, much of the market place seems to be afflicted by a kind of Gresham's Law through which bad practice drives out the good and honesty as a policy often becomes too costly to maintain against a flood of misrepresentation and fraud.

Singularly, the present ethical problems of advertising and selling are not just new outcroppings of old patterns of deceit and misrepresentation. While a residue of older frauds has persisted (witness the short-weighing scandal of New York butchers this year), ethical problems have taken on new dimensions in an institutional setting which had come to reflect a high degree of reciprocal trust between the consumer and the marketer.

On the seller's side, the consumer is today given free rein to select his merchandise in self-service supermarkets, subject only to incidental checks to prevent shoplifting. Consumers are given credit cards and veritably flooded with unsecured credit. Producer guarantees on a "money-back-if-not-satisfied" basis have increasingly come to feature merchandising practice. Such devices applied to a society in which consumers were generally unworthy of trust could quickly bring monumental producer losses.

Equally on the side of the consumer, a new degree of trust of sellers had found institutional form. Household scales have passed largely into disuse with the advent of prepackaging and of stamped weights on packages. Even the butcher's scale is disappearing into the back room as

precut meats emerge in refrigerated counters. Preweighed and pre-measured goods, boxed or canned, are often not checked by the consumer when bought. On the quality side, greater consumer trust has been placed in brand names, fortified by advertising, as a quality certification. With branding now applied to ever more complex mechanisms, the consumer assumption, upon purchase, is that the article will fulfill the expectations embodied in the advertising and will be both durable and safe. Lurking dimly in the background of this trust is a faith that the seller must tell the truth, and that if he does not, existing policing agencies, private and public, will somehow speedily bring him to account. In a word, consumers have developed a faith—even an expectation—that business ethics and government agencies together will exercise legal and moral discipline upon manufacturers—a discipline which will compel accurate labeling, adherence to minimal standards of performance, and the fulfillment of product warranties. While the consumer typically observes but few external evidences of such discipline in the form of government standards, grades, and inspection stamps, he assumes that they are present.

II

In recent years, however, consumer confidence in advertising and selling has been badly shaken. The present crisis has brought about much soul searching on the part of businessmen, advertising executives, media owners, regulatory agencies, and political leaders. This soul searching is of cardinal importance because many of the offenders in the dock of public opinion have not been the familiar outcasts on the unethical fringes of business; instead, many leading sales and advertising organizations have been directly involved, not a few of which have long paid lip service to high levels of business ethics.

Five of the most noteworthy areas of contemporary criticism have been the following:

1. *Deceptive Designations of Quality.* At the outset, it should be recognized that branding together with "puffery" are today openly designed to emphasize as significant a host of meaningless product differentiations. Perhaps the most succinct illustration of this aspect of the function of advertising was given by Martin Mayer in his book, *Madison Avenue, U.S.A.*—a recent volume about advertising which, by the way, was lauded by the advertising trade press. Mr. Mayer quotes an advertising executive as saying: "Our problem is—a client comes into my office and throws two newly minted half-dollars onto my desk and says, 'Mine is the one on the left. You prove it's better.' "[3]

But deceptive quality designations go considerably beyond the cre-

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[3] Martin Mayer, *Madison Avenue, U.S.A.* (Harper and Bros., 1958), p. 53.

ation of false distinctions in quest of sales. In furniture, for example, true wood names are placed on fabricated materials. In tires, misleading quality terminology is compounded: not only have first, second and third line tire designations been meaningless except as distinctions within a given brand, but even within the brand other and still less meaningful quality marks are superimposed. If, for example, you wish to purchase the second line tire within one major brand, you buy the "super de luxe." The range of quality designations for this particular brand of tires ran, as follows, in 1958: Deluxe Champion, Deluxe Super Champion, and Super Champion as first, second, and third line designations. In automobiles the trick has been to degrade models within the line. For example, the top Chevrolet, the Bel Air, has now been shifted downward into the middle bracket. The same fate has been accorded to the top Plymouth, the Belvedere. Ford, not to be outdone, has demoted its Fairlane from the top to the bottom designation.

In food products, Grade A is sometimes not best, but is superseded by double or triple A. Giant olives are comparatively small (colossal is the top grade) and some foil-wrapped frozen foods labeled "Extra Fancy" turn out to be Grade B. In furs and in textiles, in recent years, the misuse of quality designations resulted in such chaos that sellers themselves turned to the Federal Trade Commission for policing through labeling laws. But in a wide range of goods, from upholstery to light bulbs, misleading quality terms constitute a growing threat to orderly marketing and effective price competition.

2. *Fictitious Price Designations.* The phony list price has today become a byword. Its rapid advance in the markets of the fifties probably stemmed from the over-market ceiling prices established for a number of consumer durables under the Office of Price Stabilization during the Korean war. After the war, the fictitious list spread rapidly and widely throughout many consumer goods categories. A bewildering array of actual market prices has resulted.

In a recent limited survey of prices for de luxe refrigerators, on one specific current brand and model, the buying prices taken from twelve different cities ranged from $430.00 to $720.00 for a product which listed at $629.95. This survey was made, to be sure, in June, 1960, when the drop in the purchase of consumer durables had begun to hit with chilling force.[4] But a year earlier and before the current recession, a similar situation was found in tires. Market prices ranged from $36.00 to $67.00 in one branded premium tire, and from $49.00 to $87.00 in another. More or less the same ranges, percentagewise, were found in the other tires included in the survey.[5]

[4] *Consumer Reports*, Sept., 1960.
[5] *Consumer Reports*, July, 1959.

Given knowledgeable consumers, the problem of fictitious prices would disappear in a competitive system. Prices are ideally the expression of a relationship between fluid, dynamic, and multiple forces. With the advent of preticketed prices (or prices otherwise listed as if they were stable), we have introduced illicit sirens which have led to many dubious ethical practices. Fringe operators have entered the picture to outpace the fictions of their more conservative competitors. The practice became widespread of putting something like a $100.00 list on a $30.00 item in the expectation that, to a portion of the buyers, a marked-down $60.00 price from the $100.00 fiction would look like a bargain. If a list 40 per cent above market price acted as a successful barrier to vigorously competitive prices, then a higher list might, in the absence of quality designations, serve even more effectively. In recent years, it has not been uncommon for goods to be advertised for sale for as little as 25 per cent of a fraudulent preticketed price. It was at this point that the current drive against fictitious pricing gained real momentum. Yet it should be observed that many of the most ardent spokesmen against fictitious prices are themselves placing their own list prices 10 per cent to 40 per cent above actual current urban selling levels.

3. *Deceptive Packaging.* In self-service retailing, the package, as you will read time and again in the sales promotional trade press, has become a salesman. And you may be surprised to learn what a deceptive talker a package can be. There is today emerging an overwhelming body of evidence of the deliberate selection of nonstandard package sizes and weights by many companies in an effort to avoid rational consumer comparison in terms of price per ounce. Here, for example, are a few samples recently reported by representative consumers:[6]

Huntsville, Alabama:

A major manufacturer makes "instant milk" in several sizes. The "large" box contains enough powder to make 14 quarts; sells for $1.19 a box at a cost of 8.5¢ a qt. The "economy" size makes 20 quarts; sells for $1.79 a box, at a cost of 8.95¢ a quart. . . .

Los Angeles, California:

My supermarket displays 4 brands of instant mashed potatoes, all selling at 33¢ a package, and *all* claiming in large letters to serve 8 people. Apparently the manufacturers have varying ideas of how much potatoes 8 people will eat, for the actual net contents of the packages (printed in very small letters, of course) are as follows:

(A) Mashed potatoes ....7¼ oz.
(B) Instant M.P. ....7⅕ oz.
(C) Mashed potatoes ....7 oz.
(D) Whipped potatoes ....5½ oz.

Strangely enough (or not so strangely) the less potatoes in the package, the larger the package. The (D) package, which contains only about ¾ as much as the (A) package, has a cubic capacity *three times larger*.

[6] These illustrations are drawn from letters to *Consumer Reports*. Company names are omitted.

Moravian Falls, N.C.:

One of the most flagrant deceptions I have seen is X's Instant Coffee in a jar containing 5 oz., same size jar or container as other Instant Coffees containing 6 oz.

Of course a search on the label will reveal that the contents are 5 oz., but this notice is not very evident! And probably a good portion of the public is attracted by seeing X alongside the same size containers of other brands, with a price tag a good deal less. Prices vary from time to time on all the brands, but I have noted that the per ounce price is about the same with X. . . .

Biloxi, Miss.:

I have been paying from 75 cents to 85 cents for 4 oz. jars of decaffeinized coffee. Recently my husband purchased the last jar and he came home delighted that they are putting out the new economy size. On checking same, I note that the new economy size costs more than the regular size, $1.08 a jar for 5 oz. It costs 30¢ more for the extra ounce.

Within the grocery industry, such manipulations of package sizes are known as "packaging to display," or "packaging to the competition," or "packaging to price." In answer to consumer complaints, manufacturers have explained quite clearly what these policies mean in practice.

Here, for example, is what a deodorant company wrote to a consumer distressed by the deodorant's oversized container:

In regard to your complaint that the X carton is larger than the bottle which it contains, our reason for packaging the product in this way is simply to gain maximum visibility for it on the retailer's shelf or counter. . . . Within the obvious limits of common sense and legal requirements, we have made the X carton large enough to provide it with the strong display values which are so necessary to its sales vitality.

Thus through recent merchandising practices, benchmarks of quantity are, like those of quality, being subjected to steady erosion through manipulation to add to "sales vitality." It would perhaps be appropriate to conclude the discussion of this particular aspect by quoting the opening sentence of an editorial from the October 25, 1960, issue of the *Chicago Daily Tribune:*

As one who sometimes eats a doughnut for second breakfast, we were interested in the announcement of the Donut Institute that a quarter inch larger hole has been decreed in the cruller for 1961 "as a sign of good times to come."

4. *Built-in Obsolescence.* This is a complex area where neither adequate data nor accepted theory exist to guide ethical evaluations. The line between waste and progress, between social cost and technological advance, can probably not be closely drawn. It is, however, abundantly clear that attempts to speed consumer replacement of goods during the fifties has had a detrimental effect on consumer confidence. The current popularity of Vance Packard's latest book, *The Waste Makers,* in itself attests that fact. Certainly concentration on product change as a sales tool has added a new dimension to compound the effort demanded of consumers in weighing rationally their utilities.

One may perhaps conclude that built-in obsolescence as a concept has not only involved engineering for replacement sales but also sales persuasion techniques designed to endow a wide range of goods with

highly ephemeral time-use values. The social costs of this effort in terms of wasted material resources and of uneconomic outlays for credit, inventory and warehousing, insurance, product display shelf-space, packaging material, trucks, retail plant capacity, manpower, and so on, are incalculable.

5. *The Oppressive Volume and Questionable Standards in Advertising.* It would be difficult to affirm that over-all advertising standards have declined from their level of thirty or fifty years ago.[7] But such a demonstration is not required. The sheer volume and frequency of the impact of this type of sales promotion in the current market raises serious questions. Even if every advertising claim were scrupulously honest, fully informative, and made with impeccable taste and delicacy, the 1,600 separate selling impacts per day that the advertising industry estimates are received, on the average, by each and every one of us are, simply, much too much. Yet the content of today's 12 billion dollars worth of advertising cannot in all accuracy be described as fully honest, fully informative, and decorous.

One may applaud the ingenuity of advertisers who today create from their imaginations new miracle product ingredients and virtues, some even unknown to scientists, and who fake scientific comparisons to demonstrate the television superiority of their brands. To the economist, however, the emergence and persistence of such practices spells the channeling of market demand toward the unscrupulous seller and the placing of a premium upon deception. Advertising has indeed become so characterized by superlatives that the thoughful person is left with the logical reflection that, if all are best, where is the worst? Social custom has long granted the seller a right to a limited degree of puffery, of enthusiasm for his own wares. Adult consumer choice has over time adjusted itself to and tempered such claims, particularly when puffery emerged on the printed page. Those who promised miracle results found their appeal increasingly limited to those media appealing to the less well educated.

The advent of radio and television lent, however, new dimensions to consumer deception. Experts in human persuasion were given direct entree to the home. There the prestige of the entertainer and the artistry of the advertiser could be blended into a visual and oral demonstration of immense power. Few advertisers have employed broadcasting with care and restraint. In the absence of adequate enforcement of the imperfect laws governing deceptive advertising, the air quickly be-

[7] That many nineteenth- and early twentieth-century advertising abuses have abated has been suggested by such illustrative studies as: Stewart H. Holbrook, *The Golden Age of Quackery* (Macmillan, 1959); Edgar R. Jones, *Those Were the Good Old Days* (Simon & Schuster, 1959); Frank Rowsome, Jr., *They Laughed When I Sat Down* (McGraw-Hill, 1959).

came the haven for the modern pitchman who created widespread and deep resentment among listeners. Unlike newspaper advertising which, if undesired, could be skillfully dodged by the accustomed eye, television commercials became an unavoidable assault on the individual as they carried their overburden of messages for alkalizers, cigarettes, beers, and assorted cosmetics.

To be sure, the radio and television industry when castigated by the press for its recent derelictions could and did point to the fact that newspaper and magazine proprietors were themselves living in a highly vulnerable glass house. Publishing standards had, to be sure, been subject to deterioration as the competition of broadcasting for advertising revenue caused copy deficiencies to be overlooked. Moreover, the rise of public relations specialists in co-operation with advertising agencies have long been prone to filter favorable comments concerning advertising products into magazines and newspapers as well as radio and television—all under the guise of news. Such copy has not infrequently won a favorable reception because of advertising accounts linked to the disseminating agent.

Ethical and social responsibility would seem to dictate especial care in those advertising media designed to reach and influence children. Yet when one observes sales promotion practices in this area, a singular social phenomenon emerges. For a nation which yields to none in its professed and actual concern for the welfare of its youth, the patience we have exhibited over the years in the face of mounting commercial exploitation of the child is truly remarkable. No secret is made of the objective of those who refer to our children as "the child market." Advertising has even extended into the classroom where the present-day teacher is flooded with advertising appeals, tailored for student use. Advertisers wish to instill brand loyalty on the impressionable, uncritical mind. Sellers of candy, "coke," chewing gum, comic books, and cigarettes wish to share in the weekly allowance. And moneylenders are seeking a cut of the baby-sitting income through their promotion of teenage credit.

III

A far more fundamental indictment of the conditioning effect of advertising has been made by Professor David M. Potter, who contends that advertising wields immense power in determining the basic value pattern of our society. "Advertising," he states, "compares with such long-standing institutions as the school and the church in the magnitude of its social influence."[8] Its central thrust, Professor Potter suggests, is that of augmenting material consumption: "It has no social

[8] David M. Potter, *People of Plenty* (Univ. of Chicago Press, 1954), p. 167.

goals and no social responsibility for what it does with its influence, so long as it refrains from palpable violations of truth and decency."[9]

The Potter indictment treats as superficial the criticism of advertising on the ground of bad ethics or bad taste. He imputes to the industry an essential materialism which diminishes the range and variety of consumer choice and tends to "make the individual like what he gets."

Professor Potter's view of advertising's influence is, in my judgment, distinctly misleading. Whatever the shortcomings of this dynamic institution (and they are many), one must acknowledge that advertising is certainly no single-directional force which narrows the range of choice and spreads satisfaction with existent material values. Instead, advertising simultaneously invites us to cling tenaciously to established products and to discard them in favor of innovation; it invites us to save and to spend; it invites us to share an ever widening range of values, some materialistic, some nonmaterialistic. Its present imbalance is as much due to the reticence of those cherishing cultural values to employ it as to the insistent demands of commerce. Custom, tradition, and law often inhibit its use. Yet one can scarcely indict this significant segment of the communications industry as lacking social goals because it is today principally employed in the sale of goods and services. Advertising, like fire, may be employed for highly varying purposes. As an industry, its purpose is to communicate ideas impellingly, not to create a social order which will have any unique emphasis. If today much of advertising is blatant, materialistic, and distorted, the problem is not one of preaching to its practitioners the necessity for a "social goal"; it is rather that of tackling specific abuses which have grown up, to the end that the industry will in the future have higher ethical standards.

IV

It has thus far been possible to do little more than to suggest sketchily that grounds do exist for giving heed to the advertisers in their judgment that a new dedication of the industry to the inculcation of social and ethical responsibilities is urgently required. Unlike the advertisers, however, who propose once again in the sixties an approach that has repeatedly fallen short during the past half century, I think the time has come to explore other possibilities, to seek for a closer fit to mid-twentieth century problems in the institutional framework designed to preserve a market environment capable of fostering free and rational consumer choice.

As early as 1912, Wesley Clair Mitchell presented the kernel of the problem with which we are dealing—a problem which, since then, has

[9] *Ibid.*, p. 188.

spread like a jungle growth. In his essay, *The Backward Art of Spending Money*, Mitchell detailed the difficulties faced by the consumer of his day in a market place where new and highly processed goods were claiming an increasing share of family income. "Surely," he wrote, "no one can be expected to possess the expert knowledge of the qualities and prices of such varied wares. . . . The single family can no more secure the advantage of [specialized knowledge] in caring for its wants as consumers than the frontier family could develop division of labor in production."[10]

Hence, even before the first World War, the bewildering complex of values was becoming evident. This was written before the automobile achieved a mass market which became, in turn, a captive market for gasoline, oil, antifreezes, batteries, tires, insurance, brake linings and brake fluids; before the electric refrigerator, the automatic washing machine, air conditioning, radio and television; before frozen foods, dried milk, powdered coffee, prepared baby food, cake mixes, and the bewildering variety of prepackaged, processed foods that line supermarket shelves; before rayon, nylon, dacron, orlon, and tens of other synthetic fibers; before detergents and wash-and-wear fabrics; before the infant chemical industry had begun its gargantuan growth to spawn a multiplying myriad of materials and products ranging from pharmaceuticals to pesticides, from plastics and synthetic rubber to aerosoled perfume. Modern technology in production had indeed evoked a complicated market maze from which today's family standards must be derived. Furthermore, modern industrial technology has in two short generations sent a mass migration from the countryside into large cities. This rapid and profound change of a way of life wiped out a backlog of familiarity with goods based on custom and family lore. Into the resultant vacuum in product experience, advertising was force-drafted in a few years from small-scale space peddling into a 2 billion dollar industry by 1920, and into an 11 billion institution by 1960.

From this vantage point of hindsight, it is hard to see how it could have been otherwise. There did not exist then, nor does there yet exist today, any other source of knowledge about qualities and prices which possesses potential resources even partially adequate to the vast need for information. And so, almost by default, the privilege and responsibility of supplying that information without which choice cannot be a rational weighing of utilities fell into the hands of sellers—sellers who are themselves frequently not equipped with the necessary technical knowledge of modern industry's flood of products and who are also, of course, totally unable to achieve objectivity and lack of bias in the in-

[10] Wesley C. Mitchell, *The Backward Art of Spending Money* (McGraw-Hill, 1937), p. 7.

formation they disseminate. Little wonder that advertising has, for fifty years, campaigned in vain for truth.

Almost instinctively, it seems, advertisers have known that persuasion is not enough; that, in the forms into which it has developed, advertising cannot ever fill the shoes it now wears, no matter how large its appropriations grow nor how many times it multiplies its daily sales impact. Moreover, no prohibitions, however drastic, against false advertising can produce the full answer to our social need. The truth is not a residue left after falsehood has been eliminated; information does not necessarily fill the gap left when misinformation has been erased.

What is required is a more positive approach, not a prohibition but a legally enforced responsibility. One might put it this way: in the place of the outmoded and now socially destructive *caveat emptor*, there must be created an institutionally reinforced basis for establishing a policy of *caveat venditor*. This would mean, briefly, not only that all information about the prices and qualities of goods offered by sellers would have the status of a warranty, but, also, that such information would be required to constitute a full disclosure. The burden of proof of compliance would have to rest upon the brander, upon the particular seller who chooses to identify the goods with his trade-mark. On non-branded goods, the responsibility would be that of the final seller.

This is not so radical a reversal of practice nor so great an innovation in approach as it might seem to be. The principle of full disclosure, for example, has been accepted for some time now in the food and drug industries. Declarations of ingredients in food and warnings against possible damage in proprietary medicines are label requirements under current law. In its last session, Congress passed a hazardous substances bill which requires full warnings on the labels of potentially poisonous products like insecticides, pesticides, cleaning agents, and such. And, as early as 1916, the New York Circuit Court of Appeals, in a famed product liability suit, *MacPherson* v. *Buick*, enunciated the principle of seller liability when Judge Cardozo stated that a manufacturer by placing a car upon the market assumed a responsibility to the consumer that rested, not upon a contract, but upon his (the consumer's) relationship to the seller which arose out of the transaction itself. As recently as last August, the Supreme Court of New Jersey, in another automobile product liability case, reinforced this earlier judgment in a decision in which the New Jersey Court stated that such cases "reveal a trend and design in legislative and judicial thinking toward providing protection for the buyer." In a recent study, *Products Liability in the Automobile Industry*, Cornelius W. Gillam, of the University of Washington, comments upon the underlying significance of such cases as these in the following words:

Formerly it was profitable to manufacture whatever one could sell, but now it is profitable to manufacture only what may be bought with a reasonable expectancy of substantial satisfaction. *Caveat emptor* has become *caveat venditor*, and no one supposes that the change is not appropriate and essential under conditions of modern industrial economy, with its impersonal markets and highly developed division of labor among the multitude of specialists.

At a time when the courts of the land have already accepted the social necessity for *caveat venditor* in the relationship between seller and buyer and at a time when the sales promotion practitioners are seeking to track down that abstraction, "truth," surely this concept will not fall with great strangeness on the ears of social scientists.

Yet legally enforced responsibility is in itself no panacea for advertising's shortcomings. There is a critical need for commonly-accepted designations of consumer goods—designations couched in terms mutually understood by buyer and seller. The legal approach to the problem must accordingly be supplemented by a basic federal inquiry into the whole field of consumer standards of identity, standards of safety, standards of performance, and over-all standards of quality—a field largely neglected since the pioneer work of Herbert Hoover or the Federated American Engineering Societies nearly forty years ago. High on the agenda of such an inquiry would be the standard designation of grades, the further standardization of containers, the extension of standards for new products, the reassessment of the adequacy of existing standards, and a review and appraisal of the current anarchy prevalent in the field of weights.

In the end, however, considerable responsibility for the maintenance of high ethical standards must still rest with the participants in the market place. No law, however stringent, can adequately define the aesthetics of cigarette or deodorant advertising or is likely effectively to limit its blatancy or pervasiveness. Equally, no law is apt effectively to discourage or prohibit planned obsolescence in automotive design. In the final analysis, much of business practice depends upon the prevalent standards of social tolerance and good taste. The newer forms of deception have already stimulated extensive discussion among advertisers and sellers and have generated widening skepticism among consumers and consumer organizations. Not a few corporations are already witnessing, and some are responding to, a significant feedback from consumers whenever they erode standards of quality and quantity and whenever they pass zones of tolerance in sales and advertising practices. While intelligent, perceptive, and articulate consumers are still decidedly in the minority, they may yet, by their influence, acquire in time a greater degree of market sovereignty. As education and organization permeate the ranks of consumers, our economic system may more nearly approximate the rationality and even the honesty that economists have long imputed to it.

THE SOCIAL AND MORAL RESPONSIBILITIES OF THE EXECUTIVE IN THE LARGE CORPORATION*

By ERNEST DALE

Ernest Dale Associates and *Cornell University*

Strike the adjective "large" out of the above topic and it is drained of most of its interest and significance. Restore it, and interest and significance are restored. Thus at the outset are we reminded of the obvious. Our concern with managerial responsibility is our recognition that whatever it is in our economic life that nurtures bigness and blunts the edge of competition also makes room for the problem of managerial irresponsibility. Among neither economists nor laymen is there much concern about the social or moral performance of apparel executives but there is concern about the performance of steel executives. All would agree that easy solutions are not at hand. Experience in enforcing the antitrust laws and growing doubts about the dynamism of an economy in which they were totally enforceable bar any confidence in a "change the environment, eliminate the problem" approach. Nevertheless, those who distrust the discipline of any force other than that of the market may take heart from the increasing intensity of the foreign competition we seem to be confronting—provided, of course, that they also believe that we will not escape the rigors of that competition by retreating behind tariff and import quota barriers.

The prevailing views on the proper scope of managerial responsibility in the large corporation today seem to be as much divided as the North-South contrast between rich and poor countries. One view appears to urge the executive to assume an infinitely broad-gauge burden of responsibilities to all of the various "publics" with whom he deals. The opposite view holds that the executive ought to stick to his last, confining himself exclusively to the pursuit of profit maximization.

On the surface, at least, the new-fashioned, broad-gauge approach appears to have much to recommend it. For, as everybody knows, the 500 largest firms control two-thirds of United States manufacturing activity. Like conditions prevail in important sectors outside of manufacturing. Concentration of size as well as of decision making is, therefore, such that the principal decisions of the several thousand top decision-makers of the large corporations are no longer wholly private in their impact. Perhaps they never were completely private, but they

are becoming increasingly public. Indeed, in the field of public ethics, the "private" executive may run into conflicts of interest so severe that this has become the single most important issue in that field today.[1]

Power Has Displaced Profits

Perhaps it is true that the appetite for additional income is never satisfied among the idle rich in a culture that honors such a class. But America is not such a culture. The propensity to participation in the labor force among our highest-income families is probably greater than among our poorest families. The evidence is clear in that executives work longer work weeks than do most persons. Why? It is my impression that most top corporate executives already enjoy income levels sufficiently high to make further increments considerably less important than increments to power. The progressive income tax schedule does nothing to weaken this impression. Apart from the pleasure of doing a job well, the pursuit of power is one of the few things in life which does not seem to suffer much from diminishing returns. As Oscar Wilde put it, "Power is wonderful and absolute power is absolutely wonderful." The shift from the pursuit of profit to the pursuit of power is probably the outstanding change of recent times in the objectives of the executive in the large corporation, and perhaps also the least discussed among his much discussed and manifold activities.

To probe the causes of this shift is beyond both the scope of this paper and the resources of its author. Suffice it to suggest that it is a nice question whether the progressive tax structure has had a greater influence than the great increases in the level of real (after-tax) income which have come to be enjoyed by corporate executives as the society has become more affluent. I mean to include the expense account component in that level of real income. Recent doubts of the economist's long-cherished notions of insatiability[2] would seem to be allayed by pointing to the 42.3 billion dollar level of installment debt now outstanding (end of October, 1960). But perhaps some measure of doubt is warranted in the executive-suite circuit. After the private airplane and the private island, how much more is there time for? Or is the rejection of increased leisure to be interpreted as additional evidence of the greater appeal of increases in power over increases in consumption?

Power and Social Responsibilities

In a sophisticated society replete with antitrust agents and political science departments, the pursuer of power is understandably inclined

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[1] See *Time Magazine*, Oct. 17, 1960, p. 102.
[2] Saul Engelbourg, "Reckoning Your Bliss Point," *Forum*, Fall, 1960, pp. 27-31.

to mask his efforts. Here is where the emphasis on social responsibilities enters in. Through the enlargement of such responsibilities or sometimes merely through encouraging a lot of talk about them, the executive is able to increase his power and appear like a noble fellow at one and the same time. Thus universities, colleges, hospitals, television cultural spectaculars, and so forth become the recipients of corporate largess and in the process usher the corporate executive into the board rooms of new domains. Among the undesirable aspects of this is the probability that in most instances the executive finds himself entering a field in which he lacks special competence but is nevertheless given special heed. Even when the donor makes an effort to avoid exerting his influence, the recipient may nevertheless seek to please him by spending the gift in his image. This is not an argument against business donations, for such an argument could turn only upon an appraisal of the costs of alternative sources of funds. It is meant to challenge the illusions of would-be do-gooders among corporate givers.

It may not be exaggeration to suggest that the wider the corporate executive defines the scope of his social responsibilities, the more apt is he to end up being hated instead of loved. This is most clearly in evidence in the international field. Some companies with investments in primitive economies, having proclaimed their social responsibilities in the loudest terms, have come to be regarded as paternalistic colonial powers. Beneficiaries find their presence so intolerable that they try to run them out of the country. That the action is irrational and defeats the economic aims of the nationalist forces doing the chasing does not render it wholly unexpected.

Yet the executives who donate to universities, colleges, hospitals, and so forth and those who have figured in the overseas tragedies that are threatening to multiply, for example, in Latin America, have at least attempted to translate their concept of their social responsibilities into action. Others merely talk about them amid like talk about their "corporate souls and consciences." This makes for nice articles and glowing after-dinner speeches when the haze of alcohol and cigar smoke support an atmosphere of friendliness and fuzziness. In the cold reality of the next morning, however, there are no yardsticks left over to measure the proper reach of social commitments. And usually there are no commitments to carry out the good thoughts that ennobled the night before. More to the point, among some such executive diners one senses that there never is any intention to get beyond big talk. Thus social responsibility may merely serve as a thinly disguised idealogical weapon to retain power for "daddy who knows best." The more the top executive succeeds in talking up his self-styled role as arbiter between labor and the consumer, between farmer and nonfarmer, between the

public and the artist, even between public and private interest (witness the recent doings in the television industry), the more power may he collect in his office.

But if we are to be true to our most fundamental traditions, we must insist that power have some place to which it is answerable. As is well known, this tradition is not being honored. Increasingly the top managements of large corporations are beholden to themselves alone. In the short run, there is little in the way of effective machinery to check on the use or abuse of power by corporate executives. In the world of blue chip oligopoly, competition, publicity, and public opinion may at best be long-run forces, and the run may be very long for managers who enjoy surpluses accumulated by their predecessors, who serve as their own auditors, and who select their own boards of directors. This relationship has two aspects: (1) As already observed, the more successful corporate executives are in gaining acceptance as dispensers of social justice (with other people's money, to be sure), the more serious is the problem of accountability for the power they wield. This follows, for their power will be that much more extensive. (2) The need for independent check is even greater and hence its lack even more important in the realm of executive actions relating to the discharge of social and moral responsibilities than it is when executives stick to their own backyards and concentrate on business affairs. While I happen to think that those who argue that the tradition of professionalism in management is a substitute for independent review have overstated their case, at least there is some kind of a case that can be made. But clearly it cannot be extended to cover the actions of corporate managers as they pertain to the affairs of colleges and other such assorted matters unless we are to suppose that American managers are all Leonardo da Vincis.

The Case for a Return to Profit Maximization

The foregoing reflections point to a reconsideration of the old-fashioned criterion of judging the exercise of managerial responsibilities on the basis of optimum utilization of investment as measured by its rate of return. Apart from the obvious and great advantages of being legal (when anything else is of doubtful legality) and at least roughly measurable, this criterion also tends to make coincidental much self-interest in the pursuit of profits and the discharge of social and moral responsibility; that is, reliance on the criterion of profit maximization need not lead to reaction. What is in the interest of the individual executive is often in the interest of the company, particularly if his compensation is arranged accordingly. Also, it is often in the interest of the various publics with which the company deals. This is another way of

saying that the service of these interests ensures the best use of the company's resources.

The steadfast pursuit of profit in an institution that contemplates perpetual life exercises a powerful negative sanction. For if the executive fails to take account of factors apparently outside the business, his long-run profits may suffer. Thus if his employment record is unstable, he is likely to encounter labor trouble. If he treats his suppliers unfairly, he may run short of vital items at inconvenient times. If he fails to eliminate unpleasant odors, excessive noise, or industrial eyesores, the community may shun his works, discourage employment, and ultimately pass ordinances against him. If he unduly raises prices, he may be called before a Congressional committee. It is these potential threats to long-run profits that make the truly responsible executive recognize his proper obligations. Experience has taught industry that compliance is better than compulsion. Thus adherence to the criterion of profit maximization need not preclude the fulfillment of social responsibilities. Concededly, the tough-minded board member could hardly be expected to distinguish those corporate actions in fields tangential to the business of the enterprise which were undertaken in the pursuit of long-run profit maximization from those prompted by an executive's craving for increased power. But at least general agreement on the overriding importance of the profit yardstick provides a basis for questioning corporate policy and confines the executives to their proper frame of reference.

Unresolved Problems That Remain

Yet there remain numerous unresolved differences between private and social responsibilities of the executive apart from divergencies arising from monetary and employment philosophies and the broad national allocation of resources. To what extent can differences be resolved by resort to the exercise of moral responsibilities?

There are two principal aspects of moral responsibility that the executive in the large corporation must distinguish. Helpful in this connection may be the lines laid down by Immanuel Kant: "imperative" and "hypothetical" morality. Imperative morality is obligatory through the framework within which the executive has to work. For in order to justify his existence by promoting optimum utilization of investment, he clearly must fulfill his contracts and keep his promises. He must rigorously abstain from taking personal advantage of his position, so as not to violate his trusteeship position to the stockholders and forfeit his claim to legitmacy. Since in a number of recent instances questionable advantages have been obtained by and through executives in a position to influence contracts, it is important to set out

some specific standards of conduct that can guide the executive beyond the potential elasticity of his conscience. The following guide to executive conduct toward suppliers set out some time ago by the Gulf Oil Corporation may serve as such an example of moral criteria in a specific field:

1. All officers and employees of this Corporation or of any subsidiary of this Corporation must deal with contractors, suppliers and customers, and all other persons doing business with the Corporation or with any of its subsidiaries in the best interests of the Corporation and of its subsidiaries without favor or preference based on personal considerations.

2. No such officer or employee shall seek or accept, directly or indirectly, payments, loans, services, excessive entertainment and travel, or gifts of an extravagant nature, from any individual or from the representative of any business concern doing or seeking to do business with the Corporation or any of its subsidiaries.

3. No such officer or employee shall do business with a close relative on behalf of the Corporation or any of its subsidiaries.

4. No such officer or employee, with respect to any other corporation, partnership or business concern, shall own any interest such as might tend to influence any decision that such officer or employee might make with respect to the Corporation or any of its subsidiaries.

5. No such officer or employee, while employed by the Corporation or by any of its subsidiaries, shall acquire any oil or gas royalties or any mineral interests except where acquired as incidental to the purchase of land. In no case can such acquisition be made if based on information obtained from Gulf sources.

6. Any such officer or employee who violates the above stated policy shall be subject to disciplinary action, including possible discharge.

Moral obligations going beyond those required for legitimacy were called "hypothetical" by Kant, in that they may either strengthen the claim to and the maintenance of legitimacy or go beyond it and be independent of it. If management can merely be persuaded to be interested in the furtherance of its own claim to legitimacy, two hypothetical or voluntary moral responsibilities appear to be outstanding.

First, management must encourage free discussion among its own executives of those issues that are not predetermined and are not immediately in need of execution. The greatest single bane of management today is its growing absolutism, its refusal to discuss or listen to different opinions. This failure deprives the company of the only effective way of reducing error on the one hand and of bringing about progress through change on the other. Free discussion has been declining within management for many reasons. Competition is no longer as punishing or as panicking as in former times. The mistakes of an increasing number of large corporations tend to resemble more those of large states in that they take time to show up. Sometimes they do not show up at all because others tend to make even worse mistakes. Inside the corporation, group decision making tends to fudge accountability. "Follow the leader" decision making tends to make heroes of those who have copied the right leaders at the right time. Mistakes are excused on the ground that if the leaders made the same mistakes,

what more could be expected of the followers? And any rebuttal power of stockholders and boards of directors may be squashed by the device of having management serve simultaneously as the board of directors or controlling it effectively by various now familiar means.

So large a degree of power within the corporation, allied with great economic resources, is subject to potential abuse or at least is conducive to the commission of needless errors. Hence the need arises, second, to create rebuttal power inside the corporation by permitting an atmosphere of free speech and outside the corporation through the counsel of groups or individuals who are independent of the internal management. Stockholders, unfortunately, have been increasingly unable and unwilling to be management's "still voice of conscience." They may elect their representatives, but they do not select them. They may approve, but they do not propose—much less oppose. The "new stockholder tycoons" of the financial institutions that hold about one-third of the equity values on the New York Stock Exchange are new all right, but they are in no way tycoonish in holding internal managements to their responsibilities. Almost always they would rather sell their shares than speak up. The board of directors staffed with insiders is merely the other side of the same coin. Outside directors, while they undoubtedly make valuable contributions, do not as a rule have sufficient knowledge, power, and time to ensure genuine accountability of internal management's responsibilities.

But there are possibly some ways of replacing the old "dinosaur" stockholdings of large accounts, whose spokesmen made great contributions to long-run profitability. This might be done, were its legality assured, through the formation of a nationwide association of institutional and individual investors. Its purpose would be the selection and appointment of representatives to the boards of companies in which the combined holdings of the association are large, either absolutely in terms of total dollars or relatively as a proportion of issued shares. Directors would be chosen for outstanding technical competence and be made independent of the companies on whose boards they serve by paying them well and by having each of them sit on several boards.

Such professional directors would not be perfect substitutes for owner-directors, but they might champion a more long-run approach to profitability and a more intelligent approach to the question of the corporation's social responsibilities than now prevails. And their vote might be strengthened further by the Dutch device of strictly separating board of directors (the Dutch "supervisory control") from internal management (in the form of a "board of management"). The former is staffed entirely by eminent outsiders whose role is to represent the stockholders effectively by broadly holding the internal management to

its responsibility, ultimately by the threat of dismissal, and by umpiring basic differences among the members of internal management through two "delegates of the board."

The device of an "advisory council" (e.g., Unilever's "Beirat") might be used as a means of strengthening the exercise of social and moral responsibilities on the part of the internal management. Such a council might consist of eminent public figures from politics, finance, science, economics, and so forth who would meet informally with the internal management at regular intervals to act as an unofficial "devil's advocate," counseling on problems submitted by internal management, drawing attention to social and moral shortcomings or to future problems not yet adequately faced. There would be no obligation by internal management to accept such advice except insofar as the council would exercise influence rather than power. Perhaps a more effective substitute for the rebuttal power of what I have called the "partial proprietors" has been developed through publicity in Scandinavia, characteristically the home of the "Ombudsman" or "Safety Valve" vis-à-vis governmental misadministration. Under Swedish law corporations have to permit an audit by a board of four auditors, assisted by a small internal staff. These four are outstanding men in the public eye, with business analytical experience. They are elected by the shareholders (there must be representation of all principal shareholders), paid for by the stockholders, and report to them at their annual meeting. The board of auditors has a number of discussions with each major executive—private and confidential—at which promise and fulfillment of company plans and their reasonableness are analyzed. Competence, attention to detail, and independence on the part of the auditors tend to insure constructive analysis. The feeling of the board's recommendations and the Swedish equivalent of the SEC insure a record of public knowledge that is forcing managements to remain on their toes.

Companies with this type of independent review might then become leaders and pacesetters and perhaps force the reluctant ones to follow suit—if not by the force of competition, then at least in the name of respectability.

Conclusion

Having said all this, let me acknowledge immediately that merely admonishing management to accept an independent review of one of the types suggested above will appear weak to some. Admonition is seldom effective. Management itself will naturally be reluctant to admit to any conflict—actual or potential—between itself and its stockholders (or anyone else, for that matter). And few persons of power are likely voluntarily to accept any curbs on their own actions. Ingrown manage-

ments, in fact, are likely to view the whole problem as one of "public relations" and attempt to provide the shadow instead of the substance of solutions. As for institutional investors, they have so far not shown much interest in stepping into the gap.

Hence, our proposals to make self-serving managements more accountable to stockholders (as well as to the public) are unlikely to be taken seriously either by managements or by institutional investors. But would the ultimate and only alternative to self-reform be more effective and more desirable? Clearly that alternative is federal intervention to enforce social and moral responsibility, going possibly as far as direct or indirect government participation in the deliberations of boards of directors.

One need only recall the many government regulatory agencies that now possess powers of intervention. In most cases, because of the almost invariable tendency of the regulators so to identify themselves with the producer group or dominant pressure group involved as to be indistinguishable from them, real representation of the consuming public is conspicuous only by its absence. And in this *custodiet ipsos custodes* who is there to admonish the monitors? Theoretically, of course, they are accountable to elected officials who appoint them, and the latter are accountable to the general public at election time. But the connection is tenuous in the public mind, and the actions of administrative agencies do not appear to sway many votes. That is the basic weakness in the idea of waiting for the government to step in and to exercise the independent review of management that is not now being exercised.

Accordingly, the continued admonition of management to preserve and expand the independent review of its own actions—and of institutional investors to play a part—seems worth while, especially since it must ultimately be realized that the only alternative is public intervention of a kind that but rarely keeps the public interest paramount.

THE INFLUENCE OF MORAL AND SOCIAL RESPONSIBILITY ON SELLING CONSUMER CREDIT

By Arch W. Troelstrup

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Introduction

Let me impose on your good nature, at the outset, and make a few confessions. This assignment turned out to be a very difficult one. As of now, I am of the opinion that only two types of people would consent to tackle it: an inexperienced neophyte (not necessarily too young to know better) or a person who has so many scars on his hide that a few more scars would not hasten his exit. I plead guilty on both counts.

Regretfully, the academic world has not been a source of great strength in this controversy—either intellectually or morally. The academic material in this field was not made easier by the fact that most of the studies made in recent years have been financed by grants from consumer lending agencies. Nor do the five volumes entitled, *Consumer Instalment Credit,* put out in 1957 by the National Bureau of Economic Research for the Board of Governors of the Federal Reserve System, get to the heart of the problem. George Katona's analysis of the functions of installment buying in our society was informative and helpful.[1] Dr. Katona's recent book entitled, *The Powerful Consumer* (1960), and the report entitled, *Attitudes Toward Sales Finance Companies and Installment Credit,* issued in January, 1960, by the Survey Research Center at the University of Michigan, was useful particularly on consumer attitudes on interest rates and the cost of credit as between lenders. The 898 pages of *Hearings* on Bill S. 2755, entitled *Consumer Credit Labeling Bill,* were rewarding in a multiple sense, but particularly for the disclosure of finance charges in connection with extensions of short-term finance credit. For those of you who struggled through the 898 pages of *Hearings* on S. 2755, my admiration and sympathy. When my vision became blurred from the effort to see the ethical and moral picture clearly, I found myself returning time and again to Dr. Howard Bowen's well-known volume on *Social Responsibilities of the Businessman* (1953). For sheer forthrightess and intellectual honesty, in this controversial area, a medal for bravery and courage should go to another economist, Dr. Mildred Brady, presently on the staff of Consumers Union. Her articles in *Consumer Reports* gave me the heart to finish the job.

[1] Comments on Bill S. 2755, dated Mar., 1960 (mineo.).

Like the historic division of Gaul, my story has three parts: (1) The Nature of the Problem, (2) The Moral Issues, and (3) What Needs to Be Done About It.

The Nature of the Problem

Part of the problem is succinctly stated in the title of this paper: "The Influence of Moral and Social Responsibility on Selling Consumer Credit." Selling consumer credit is, of course, selling debt—a commodity that would generally be an undesirable one as far as the buyer is concerned. The techniques of selling debt today, however, are such that the nature of the commodity for sale is concealed from the buyer. In the first place, a large percentage of those who now owe short-term consumer debt do not really consider themselves as debtors. They look upon themselves solely as buyers. In other words, the loan is disguised as a sale. This is a significant change from the past—a change from a financial device to a merchandising tool. In actual practice, little remains in what we term consumer credit today of the theory so fondly elaborated upon by Seligman back in the twenties. Furthermore, the goods are used as an inducement to the borrower, who is unaware that he is borrowing and undertaking the status of a debtor. Then, too, the returns in many instances to the seller of the goods and debt are more liberal for the debt than the goods. In short, the goods become a little like a premium offer where the significant transaction is not that of buyer-seller but that of lender-borrower. Furthermore, the debts induced by merchandise have a peculiar nature as debts today. In the first place, the concept of collateral has almost evaporated. Before World War II, it was assumed generally that the goods sold on installment terms functioned as collateral for the debt. The lengthening of the contract time in recent years to thirty-six months (and even fifty months), together with the rapid obsolescence of consumer durables, has eroded the concept of collateral from these contracts. It is not necessary to belabor the fact that goods do not serve as collateral in these days when installment debt is used to finance services such as travel and all soft goods as are now available under present revolving credit plans in department stores and mail order houses.

This does not mean that the sellers of debt are insane, however. Although the goods used to induce the loan represent no surety to the lender, the legal status plus the expectation of full employment does. As long as an individual is employed or as long as he has any material assets whatsoever, the short-term consumer debt he contracts as a buyer, not a borrower, constitutes legally a potential lien on any of his resources. Garnishment laws which are a maze—even to a lawyer,

since there has been no serious study of them—permit lenders disguised as sellers to tap current earnings. And the policy followed in the granting of judgments in the courts against buyers who are debtors but actually do not know it, is such that any possession the buyer-borrower has can be subject to a lien or seizure on court order. No longer does a retailer "carry the consumer" over a period of time, as the general store once did the farmer between seeding and harvesting. Only the old-style thirty-day charge account offered by department stores can in any sense be called a retailer carrying service, and this is the only form of consumer credit which has failed to increase. Typically, retailers turn their installment paper over immediately to a lending institution. Hence as soon as he drives the car around the block or installs the washing machine, the buyer has become a borrower from the bank, sales finance company, etc.—a lending institution he did not choose and with whom he made no commitment directly that he was aware of. This hiatus between the seller of the goods and the holder of the debt also means that legally the implied warranty in the sale of the goods is lost although the debt responsibility on the buyer remains firm and fast. So it is possible for a buyer to be told he is acquiring, for example, twenty yards of all-wool carpeting at $20.00 a yard, and wind up owing a lending institution $400.00 for twenty yards of carpeting *plus* the usual expensive credit costs and *plus* any legal responsibilities that may occur. Not since the days of Solon in the fifth century, B.C. (he was, you recall, the proclaimer of the first total debt moratorium), has an untutored population been faced with a new medium of exchange in the hands of promoters. In Solon's time, it was the introduction of coinage into a barter economy. Today, it is the creation of deficit-financed purchasing power by signature on a debt contract.

Since consumer credit has become precisely what the title given to me implies—a selling device—all of the evils long associated with the promoter's activity are associated with the selling of credit. Plus one other: credit is sold, not only with the buyer unaware that he is a borrower, but it is also sold at concealed and, sometimes, at unspecified prices. You and I have seen revolving credit contracts which actually state that the service charges associated may be changed from time to time at the will of the seller-lender. It does not do any of us much good to elaborate the obvious by stating that anyone who signs such a contract has a hole in his head. The facts of the matter are that people are signing such contracts. They are induced to sign them by sellers, and goods are used as the inducement. Trying to teach people how to read fine print and understand it is an almost impossible job.

So the situation we are in comes down to this: in an economy where
12 billion dollars a year are spent theoretically to induce a desire for
goods, and where sellers make verbal assurances that do not have to
be backed up in court, the device of credit at a concealed price is used
as a mechanism to push forward heavy inventory at noncompetitive
prices. Few seem to doubt that without credit there would be a con-
siderable slowing of the movement of goods through the distributive
channels into the final user's hands, but almost no one seems to be
willing to take the next step that is important; namely, through the
use of credit, the price at which they can be moved remains higher
than you might reasonably expect in a market where inventories were
heavier than effective consumer demand.

Of course, one other aspect of the nature of the problem lies also
in the sheer volume of consumer credit now extant, and an even more
important aspect of the problem lies in the uneven impact of this debt
on the population. Most of the data deals with relationships such as
that between outstanding short-term debt and disposable income; or
outstanding short-term debt and total population; or outstanding
short-term debt and estimated discretionary income; and so on. In all
instances, such calculations conceal as much as they reveal about the
problem since short-term debt appears to be heavily loaded against
particular groups in the population: (1) the young family where the
head of the house is thirty years old or under; (2) those groups in
the population who lack social experience, such as the Negroes, Mexi-
cans, Puerto Ricans, and so on; and (3) a smaller group that has been
characterized as the debt-prone—the splurgers, emotionally immature,
etc. To put it another way, the roughly 50 billion dollars of short-term
debt probably has its heaviest impact on the group of the population
comprising less than 50 per cent numerically. This is a group who
could probably not survive or remain solvent if there were so much as
a thirty- or sixty-day interruption of their cash income.

It sometimes seems to me that with respect to consumer short-term
debt, we are in a position not unlike that in the late twenties, except
that then the debt was allocated against the family operating a family-
size farm, and the rates of interest a good deal easier to bear than
those borne by consumer credit. You may remember that in the late
twenties farmers were driving with their shotguns twenty, thirty, or
forty miles to save a farm up for auction. Today the situation has been
transferred from the plains of Kansas to the streets of Kansas City.
As long as we are able to avoid the kind of economic contraction that
took place in the late twenties, we may be able to survive, but the
economic and social implications of a heavily indebted urban popula-
tion could be explosive.

On the matter of rates of interest, the facts are almost unbelievable. Some ten to fifteen states in the past four or five years have passed legislation setting ceilings on rates of interest that may be charged on installment and revolving credit. The lowest rate allowed in many of the laws as an acceptable interest charge is 20 per cent—this is in New York for installment sales. For revolving credit, the rates range from 18 per cent up to as high as 50 per cent, and 100 per cent if the account drops down to a minimum. In New York State, for example, which has the "best" law on this matter, a retailer may charge a flat 75 cents a month service fee even though the account is no more than $10.00. That comes out to a 90 per cent true annual interest rate. In most, if not all, other states the allowed minimums are higher.

Until a generation or so ago, the vast majority of our urban population had only limited experience with loans disguised as sales. For the most part, their familiarity with short-term indebtedness was limited to automobile financing and perhaps personal loans. Furthermore, there was and there still is a stubbornly-held folk myth about 6 per cent as a legal limit on interest rates. And the federal government contributes to this myth by limiting income tax deductions for payments of interest to a true annual interest rate of 6 per cent on the debt. Sellers of debt are well aware of the existence of this popular delusion about our usury laws. In their statements of finance charges for consumer loans, lenders almost always keep their percentage figures below 6 per cent. They describe their loan in such terms as 1½ per cent or 2 per cent or 3 per cent a month, or simply (and meaninglessly) as "low bank rates," or "pennies a week," or $1.00 a month for each $50.00, and so on. Then to make matters even more difficult, the time extensions on contracts are running increasingly to odd limits such as five or eight months, seventeen months, twenty-nine months, and so on. Add to these complexities the well-known and confusing factors of insurance fees, investigation fees, and other special charges and you have a system which leaves the buyer in the dark about the annual rate at which he is paying charges on his total loan. This lack of essential information does not permit him to obtain the best buy in the field of credit. As a matter of fact, George Katona, Director of the Consumer Research Center at the University of Michigan, discovered that even college graduates were approximately as ill-informed about the terms of credit now in use as the average person.[2] It seems to me that I recall reading in the *Hearings* on S. 2755 that William McChesney Martin, Jr., Chairman of the Board of Governors, Federal Reserve System, confessed difficulty in figuring out the total rate-cost of credit as advertised and sold today. Since it is frequently impossible to figure out the

[2] *Hearings* on S. 2755, "Consumer Credit Labeling Bill," p. 285.

actual charge, it is, of course, impossible to compare costs. And when one cannot compare competitive costs in the market place, how can you make an intelligent choice?

The Moral Issues

From time immemorial, usury has been regarded as a moral issue. For a long time, usury simply meant the taking back of more money than you loaned—eggs or whatever item you loaned. It was not until the last two or three hundred years that there developed a distinction between usuary and interest. In ancient times, interest and usury were immoral. There are many references in the Old Testament, such as that in Deuteronomy 23: 19-21: "Thou shalt not lend to thy brother money to usury, nor corn, or any other thing." In both ancient Greece and Rome, the demanding of interest was looked upon as immoral and in Plato's *Republic* there appears: "No one shall deposit money with anyone he does not trust, nor lend at interest since it is permissible for the borrower to refuse entirely to pay back either interest or principal." The great Cicero said, for example: "The borrower asks you for medicine and you give him poison; bread and you offer him a sword; liberty and you condemn him to slavery." It is true, however, that the Senators themselves, even the beloved Brutus, loaned money and loaned it at interest, but such traffic was never looked upon as moral even in Brutus' time.

The Middle Ages, of course, found usury among the greatest of sins, and on up through the sixteenth century the whole concept of money as sterile, hence any interest as theft, dominated the discussions. It was not until the age of mercantilism and the growing need for investment capital that any real distinction was made between interest and usury, and it was not until then that the concept of interest as a repayment to the lender for investments he might otherwise make began to be accepted. This particular concept of interest relates directly and solely to what might be called production debt; that is, debt borrowed to finance machinery, land, techniques, raw materials, or what have you upon which the borrower intended through his operations to receive a return in excess of the sum borrowed. The kind of interest that in ancient times was castigated was interest levied upon consumption debt, which was the most important kind of lending during that period. Consumer debt, of course, is precisely that—consumption debt—and throughout history any interest on consumption debt has been generally called usury and any charges have been called immoral.

It may be that most of us are not prepared to accept the judgment of the past on activities of the present. Time changes many of our opinions. Be that as it may, it does pose a knotty problem for many of

us. As a matter of fact, some of us have even found it difficult to justify interest on loans made for productive use. Having made room for some exceptions, I suspect that more of us will agree with Thomas F. Divine,[3] who justifies interest rates in our modern economy because they are set not by any individual or institution but by the impersonal mechanics of supply and demand in a free market. Hence, he concludes that interest is moral because its rates are set by a nonpersonal, nonpower source: the market place.

Professor Divine goes on to say that the cost of money, or interest, being known to the borrower, makes it possible for him to make a decision to borrow or not to borrow, and that this rational decision of borrower to borrow or not to borrow is, of course, the significant force in driving interest rates either up or down. It appears to me that the next step from the position Professor Divine takes is a logical one, and one that leads directly to the third part.

What Needs to Be Done About It?

Most of us will agree that the levying of "interest" on a loan with full disclosure to the borrower carries no moral stigma in modern times, because the amount of that interest is largely an impersonal measure for the demands of loans in relation to the amount of loanable money. But, today, we seldom have full disclosure of true interest rates. And there are other credit costs than interest. So long as interest rates on consumer credit are concealed, as they are today, by misleading representations, such as $1\frac{1}{2}$ or $3\frac{1}{2}$ per cent a month, $1.00 a week for the loan, 10 per cent fee, $5.00 carrying charge for $50.00 worth of merchandise for six months, service charge of 2 per cent a month, carrying charge $10.00 per $100.00, 1 per cent a month on the declining balance and so on, it is virtually impossible for even the sophisticated borrower to know the total credit costs in a language that can be used effectively in the market place. In no case, except for the single payment loan, will the buyer-borrower for consumption goods be made aware of the true interest rate he is paying for the loan. And figuring out the true interest rates for these competing offers is not a simple task for many of us. It should never be necessary for a buyer-borrower (even if he could perform his arithmetic, which is doubtful) to be forced to perform such complicated means to determine comparative costs between competing sellers of credit. Consumer sovereignty cannot be exercised when a meaningful final price is concealed.

It is generally accepted that there are several methods for calculating, in terms of true annual interest, the carrying charges made for goods and services bought on time. The Federal Reserve Bank of

[3] *Interest: An Historical and Analytical Study in Economics and Modern Ethics.*

Philadelphia offered in its *Monthly Bulletin*, April, 1960, a formula for making such calculations. The Bowery Savings Bank of New York published a full-page advertisement in local newspapers in which the cost of subway tokens was calculated as though they were sold on various ride-now-pay-later credit terms. The Board of Governors of the Federal Reserve System also chose a particular formula to use in its monumental study, *Consumer Instalment Credit*, published in 1957. Since there are several possible formulas available, the problem is one of selection—which formula. So far as the buyer-borrower is concerned, the all-important issue is not so much which formula is finally chosen as that some formula be accepted as standard. Without a standardized statement of total costs, consumers cannot shop for credit as they do for goods, and they must venture unprotected into a credit market where the price for this service, concealed by misleading statements of cost, varies from a low of 8 to 10 per cent in true annual interest terms up to highs above the 129.5 per cent calculated by one of Senator Bennet's panel of experts.

It does not appear to be unreasonable or illogical to request full disclosure of credit costs by use of an acceptable standardized formula. In both the courts and in the market place, the principle of the truth, the whole truth, the principle of full disclosure has been accepted as the *only* basis on which exchange and trade can be carried on. Society has succeeded in standardizing weights and measures, fabric identification, most agricultural products, medical supplies and drugs, and many other services and products. Why not credit costs? This is not a commitment of belief on my part that sellers who use goods to induce debt would become fully moral if they abided by the principle of full disclosure but it certainly would be a step in the right direction. It would be the first evidence that there has been an influence of moral and social responsibility exercised upon the sellers of credit. But as all of us know, the first attempt that has ever been made in this country to require this long-established principle of truth and full disclosure in this field was the so-called Douglas bill, and all of us have noted, in the *Hearings* on that bill that almost all sellers-lenders have rejected the proposal. These rejections based on the fears that full disclosure or an honest labeling of credit sales would depress the economy are, themselves, one of the compelling evidences of the need for acceptance of a uniform or standard formula for calculating total buyer-credit costs. These openly expressed fears are a remarkably candid admission that current credit practice is deceptive. It is highly doubtful that responsible economists, business leaders, or governmental representatives would wish to argue that deception of the consuming public is essential to a

healthy functioning of the American economic system. It, therefore, seems to me that a satisfactory solution to this present complex problem of short-term credit with its concealed costs cannot be found in futile efforts to educate the public. Nor is satisfactory solution to occur through the present system of fifty, more or less, different state laws. A satisfactory solution for full disclosure of all the costs in using short-term credit is more likely to occur if *all* the costs of credit are reduced to a common language—a language that means the same to all the people in all the states. Surely, a culture that has produced the complex computing machines has the ability to create a formula for disclosing all the costs of credit. The obstacle to full disclosure of credit costs is not our inability to construct an acceptable, standardized formula. The major obstacle lies in the short-run selfishness of man. The major problem, then, centers on the desire of business and society to sell short-term credit with meaningful and full disclosure of the total costs. And the solution will, in part, depend upon the businessman's conception of his moral and social responsibilities and, in part, upon the information, attitude, and action of the public.

DISCUSSION

HOWARD M. TEAF, JR.: As I read the three papers that you have heard, there recurred to me a question applicable to all of them: To what extent is the continuation of all this attributable to complacency?

Most of the facts and ideas presented by Professor Warne are 1960 versions of the "Chamber of Horrors" and other disclosures of the twenties and thirties. Of course, advancements in mass communication have aggravated some of the abuses. And many of the abuses are more subtle, to meet increased public sophistication. I think, though, that in the past thirty years we have made notable advances in holding the abuses within bounds. Would you trade the mislabeled or underlabeled proprietary medicine of today or the labeled fabric of today for the pitchman's patent medicine or the unlabeled shoddy cloth of yesterday?

The consumer-credit problem, too, is not new, and practices today are probably less usurious than formerly. Surely the abuses exposed by the early Russell Sage Foundation and other investigations were more offensive to the public interest than today's abuses, the worst of the old practices having become the subject of regulatory and restrictive legislation. But the newer forms of consumer credit and their more widespread use justify our still considering the abuses and the heavy burden of interest and related charges as a major social problem.

Commonly we ascribe to developments in communication, particularly mass communication, the compounding of abuses of consumer interests. But this same mass communication and various programs of consumer education present real potential for improving the consumer's position. I believe that today consumers are better buyers—if they want to be such! (Thirty years ago we did not have Consumers Union.)

In spite of concealment of many of the high costs, in price and quality, and in spite of the subtlety of many of the misrepresentations and other practices, the American public is not ignorant of what is going on. Outrageously overstated claims, in print or in television commercials, are commonly, or at least frequently, looked upon as humorous. In fact, we have instances where the advertiser himself presents, humorously, an obvious overstatement, apparently only to make an impression of the brand name on the observer's memory.

Here, parenthetically, I must except the television-viewing children. They cannot be expected to recognize overstatement and, unfortunately, are relatively "easy targets" for an increasing amount of child-directed commercials.

The public also knows all too well the high cost of consumer credit. Because of the complexities pointed out by Professor Troelstrup, they possibly do not know just how high. In any case, the known high cost is not a deterrent to use of the various forms of credit. Possibly you know, as I do, of cases where people with adequate money in the bank still purchase appliances

on installments. And we have the new credit arrangement wherein you borrow from a bank, at a high personal-loan rate, against the collateral of your own savings account, on which you receive interest at only a fraction of the rate charged on the loan.

What interests me is the complacency with which all this is accepted by the public—buyers and borrowers. Instead of exhibiting feelings of revulsion or antagonism, the public gives indications of "we couldn't care less." I suggest that this may be, practically inevitably, a derivative of our affluent society. In terms of marginal effect, taken in dollar cost or in real cost, the burden of these abuses is far less than the burden of the abuses of a quarter or half century ago.

In attributing the complacency to our affluence, I am not evading the moral issue. On the other hand, I believe that this represents a soft moral spot in our affluent society. People do not take, on principle, a hard position against misrepresentation even when they recognize it; and the misrepresenters are thus encouraged in their policies.

Complacency enters, too, into the relationships discussed by Professor Dale. Managements may be strongly influenced, especially in policy decisions related to investment and dividend distribution, by the interest of stockholders, particularly large stockholders, in stock-value increases. Here also should be mentioned the stock-price interest of company executives themselves—the management—because of stock options they cause to be issued to themselves. The measure of success is the increase in the price of the stock, which may or may not reflect an increase in earnings and which usually is still more independent of dividends. And the earnings may not respond by an amount that justifies the plowing back of past profits. Corporate relations, as Professor Dale shows, are complex, subtle, and indirect, and these characteristics, combined with the attitudes of managers and stockholders, make it easy to avoid the onus of social responsibility.

There also is a moral weakness in the acquiescence or blindness with which many corporations, including some of our largest, accept a double standard: one for their own behavior, another for their employees'. The same firm that "goes along with" the payment of what can only be called political graft would discharge one of its employees who accepted a bribe. That the payment of graft or of unearned commissions is necessary to get or to keep the business of a government body is no answer; this is only another instance of what Professor Warne calls "a kind of Gresham's Law through which bad practice drives out good and honesty as a policy often becomes too costly to maintain. . . ."

My feeling that complacency is widespread, that there is a too-general shrugging off of moral principle, leads me to believe that the public—the consumer public, the borrowing public, the stockholding public, the corporate management public—will not be aroused to direct action or to the support of legislation on economic grounds. Somehow there must be a kind of moral awakening, an arousing on principles, whether they be principles of justice or equality or just plain honesty. To get results, therefore, economists not only should study the markets and institutions wherein moral standards or social

interests are most likely to be neglected or abused but also should assume the responsibility of prodding and awakening the consciences of both business policy-makers and the public.

RAYMOND T. BYE: For some two or three decades a group of distinguished economists has been working to perfect a positive welfare economics from which all judgments of moral value would be excluded. With great subtlety they have developed a body of analytical theory that aspires to the dignity of pure science. But implicit in their reasoning is the assumption that each person is the best judge of his own welfare and that therefore the economy should be directed toward maximizing individual preferences, as expressed in market choices. The criticism that this is in itself a value judgment is now pretty generally conceded. Frankly facing this problem, an offshoot group would have us maximize a social welfare function, explicitly embodying certain ethical norms which the economist is to take as given. Whether we adhere to either of these groups or prefer (as I do) a more humanistic approach, it is apparent that we cannot have a welfare economics without ethics. It is therefore fitting (and I find it refreshing) for the American Economic Association to devote one of its sessions to the discussion of ethical problems.

Many, perhaps most, economists accept too uncritically the idea that individual choices should guide the economy and make the tacit assumption that these choices do rule the market. Professors Warne and Troelstrup have shown again (what many of us have known all along) that so far as consumer choices are concerned, this is not so. Consumers often do not know what they want, and are so effectively deceived that they could not get it if they did; so that the supposed reign of consumers' sovereignty is seriously undermined by dubious advertising and installment selling practices. And Professor Dale reminds us that individual choices do not rule the large corporations either, for here power-seeking executives pull the strings, sometimes with little regard for the interests of their shareholders, employees, or the general public.

There is no doubt about the existence of these evils, evidence of which has been so convincingly presented here. The important question is, what can we do about them?

Among the remedies proposed by our speakers are three that seem to me of major significance: standardization, full disclosure of relevant information, and, in the case of corporations, some kind of super-directorships or advisory boards of review. Mr. Warne recommends standard grading of consumers' goods and standard sizes of containers. Mr. Troelstrup wants standard methods of computing the costs of consumers' credit. Mr. Warne wants labels that clearly and reliably reveal to the consumer just what it is that he is getting for his money. Mr. Troelstrup wants the installment buyer to know exactly how much he is paying for credit when he buys on the time payment plan. Mr. Dale does not suggest the use of either standardization or disclosure for the problems he discusses, but I think he might well have done so. We greatly need some standards to say what is a reasonable salary and what is exorbitant in the remuneration of corporate executives. I do not think the market

is any better a guide here than it is in the purchase and sale of consumers' goods, for the simple reason that it is not perfectly competitive. As for the second remedy, full and fair disclosure is the very essence of the regulation carried out by the Securities and Exchange Commission and is proving very salutary in raising the tone of securities selling and moderately helpful in reducing the exploitation of shareholders by corporate officers.

I am not very favorably impressed by Mr. Dale's suggestion that corporate problems of social responsibility be solved by concentration on profit maximization and reform from within, but the suggestion for some kind of advisory boards of review (on which he laid somewhat more stress in an earlier draft of this paper than in the present draft) impresses me as having some merit. Some labor unions are using such boards to protect their members against abuse by their officers, and it might work with corporations. But I think the boards would need a means to give force to their counsel where recalcitrant corporate officers refuse to follow their guidance. This brings me to propose a further measure that could supplement and strengthen the devices recommended by our three main speakers.

I submit that we could use publicity. We have been talking here about how advertising is used to exploit the consumer. Why not use it to help him? The power of modern advertising is enormous. The sales results obtained by TV sponsors, for instance, are astounding. Think what might be done to inform the consumer, to teach him to buy with discrimination, if this and other media of advertising were turned to his advantage. And the same could be done for shareholders.

But this will not be done by sellers, who undoubtedly will continue to devote their advertising efforts to their own gain. Neither can it be done by voluntary consumers' organizations, nor by existing educational institutions, for the costs would be too great for them to finance. There is only one agency that could have both the motive and the means for so great an undertaking, and that is the state, especially the federal government, to which we increasingly turn for help in solving our social problems.

It has often been urged that there be created a federal department of the consumer whose chief would be a member of the President's cabinet. In this department would be centered all activities of the federal government directed toward the interests of the consumer. It would sponsor appropriate legislation, test products, and make public its findings, administer the food and drugs program, regulate private advertising, prescribe standards for containers, supervise the grading and labeling of merchandise, and, if my suggestion were followed, it would carry on a vast campaign of advertising, using all the modern arts of publicity—all these things to promote the consumer's welfare. The recently created Department of Health, Education and Welfare may be the beginning of such an agency. But it will need to be given broader powers and encouraged by a vigorous and farsighted President if it is to perform the functions stated.

I would also use publicity to give force to the advices of the corporate boards of review proposed by Mr. Dale. Where corporate officers act counter to their responsibilities in spite of such boards, the latter should have the

right, and indeed the duty, to publicize the facts and their recommendations. Just as publishing of the facts and recommendations of impartial bodies is being used today to bring pressure for the just settlement of labor disputes that affect the general public, so might we use this same weapon to push corporate officers toward socially responsible behavior.

DEXTER M. KEEZER: My reading and rereading of the principal papers by Ernest Dale, Arch Troelstrup, and Colston Warne has been enlightening, and I cheerfully permit myself to suspect, morally uplifting.

For new insights into (or is it on?) what are relatively new problems, I found Mr. Dale's discussion of some of the dilemmas of the executive in the large corporation particularly productive. But I also feel indebted to Mr. Troelstrup for his prodigiously fast intellectual footwork in first building up a horrendous case of immorality in the selling of consumer credit and then erasing a large part of it by the simple expedient of attributing the wickedness to impersonal and presumably amoral forces of supply and demand rather than to individual devils. And, of course, I was again impressed by Colston Warne's unflagging vigor in scouring the seamy side of the advertising and selling business. He did not mention the field of industrial advertising and selling, with which I have some occupational familiarity, nor, so far as I can tell, does his general indictment fit it. I feel he has a scholarly obligation to be more discriminating in framing his indictments, particularly since he has had such vast experience in this line of endeavor.

In studying the papers, however, I found myself trying to fit them into some broad and generally applicable concept or concepts of moral and social responsibility. In this operation I had considerable difficulty.

For Mr. Troelstrup and Mr. Warne the primary touchstone of moral and social righteousness seems to be to tell the truth or the closest approximation of it possible, which sometimes is not very close. For Mr. Dale, how economic power is exercised seems to be a more compelling consideration.

In moments of despair of reconciling our traditional morality, to which I try to cling as the guide to my personal conduct, and the actual performance of our economy, I sometimes tend toward the pragmatic view that the only feasible test of good morality nowadays may be whether or not, viewed broadly and sometimes mighty broadly, the arrangement in question works relatively well to promote the general economic welfare.

For example, as viewed against our traditional standards, I would submit as a moral offense of large proportions in the postwar period the overpayment for their services of many if not most of our run-of-mine production workers. The nature of the offense was succinctly stated by the late Sumner Slichter in a memorandum shared with me in which he wrote:

. . . the spectacular gains in productivity are made possible by investors, scientists and engineers [I would also include some administrators] yet the gains are pretty completely appropriated by labor which pushes up its wages far faster than the rise in output per man-hour. Today the process of exploitation in capitalist America is diametrically opposite to the process described by Karl Marx. Marx thought that capital exploited labor, but in America today labor exploits capital, science, and engineering. How long will the community tolerate this topsy-turvy system of distribution by which the routine workers appropriate the gains made possible by the risk takers and the innovators?

He could see no signs of revolt when he wrote in mid-summer, 1959. I have not detected any of much consequence since.

In economic terms, I do not complain much about the fact that many people get paid more and more for doing less and less. As the poet Milton observed, "They also serve who only stand and wait," so it occurs to me that in the kind of economy we have created in the United States, they may also serve who simply sit and consume.

In terms of our traditional morality, however, the problem is not so easily disposed of. As I have understood it, to each according to his contribution has traditionally been a basic moral tenet of our capitalistic economic system, as opposed to the idea of each according to his needs officially embraced by many if not most collectivists. But now we very conspicuously ignore the idea of each according to his contribution—perhaps even as much as most collectivists ignore the idea of each according to his need.

It seems to me crucially important to develop a set of moral guides and principles which make a firmer fit with the facts of modern economic life than that provided by a rough appeal to expediency and pragmatism. In fact, I think it quite as crucial as updating our economics, which was developed largely through the study of scarcity, to fit more closely the abundance which has emerged as a dominant economic fact in the U.S.A.

Inevitably these papers do little more than scratch the surface of the massive—and perhaps in large part insoluble—problem of making our moral principles and our economic performance tolerably comfortable companions. But for doing this much in a stimulating, if necessarily inconclusive and controversial, way, I am grateful to those who labored on the papers about which I have been privileged to make this slight comment.

ECONOMIC EDUCATION: CHALLENGE TO OUR PROFESSION

THIS IS ECONOMICS IN THE SCHOOLS*

By PAUL R. OLSON

State University of Iowa

Introduction

This paper is a summary report on one aspect of the activity of the American Economic Association's Committee on Economic Education: the status of education in economics at the high school level in the United States. As one step in the Committee's concern with this aspect of education, a study of textbooks used in the social studies portion of the high school curriculum was undertaken.

The study was not concerned with whether economics could or should be taught at the high school level. The number of high schools offering formal courses in economics, either on an elective or required basis, was not a matter of specific attention. The study proceeded on the basis that most high school students are being exposed to economics, or economic understanding, in courses falling within the framework of the social studies. This framework was regarded as consisting of courses in economics, problems of American democracy (social problems), and United States history. The assumption underlying the study was that the textbook is the important core in presenting economic understanding to high school students.

The study was conducted by a group of twelve economists, composed of teachers of economics at the university or college level and one professional research economist. The examination of texts was not designed to select the best text for each of the three types of courses, nor were the reviewers called upon to produce an outline of what should be included in texts for the social studies curriculum. They were only asked to examine the books from the point of view of economists seeking information on the presentation of economic topics. The comments which follow are based upon the reports of the twelve reviewers.

* The following economists served as reviewers: Economics—Francis M. Boddy, Univ. of Minnesota; Ward Macy, Univ. of Oregon; Howard Schaller, Tulane Univ.; C. Lowell Harriss, Chairman, Columbia Univ. Social Problems—Clark C. Bloom, State Univ. of Iowa; A. M. McIsaac, Syracuse Univ.; Philip E. Taylor, Univ. of Connecticut; Kenneth Roose, Chairman, Oberlin College. (This group functioned as a committee of three following the death of Professor McIsaac in Jan., 1960.) United States History—Clark Allen, Southern Illinois Univ.; Susan Burr, Div. of Res. and Statis., Fed. Res. Bd. of Gov.; Chester L. Rich, Cornell College; Lewis E. Wagner, Chairman, Univ. of Illinois.

Economics Texts

In general, high school economics texts attempt to portray economics as a part of life. Although there is a tendency to make it appealing to students on personal grounds, some attention is given to a concern of economics with the satisfaction of wants of members of the society. It is apparent, however, in dealing with economics as one of the social studies, that authors of the texts attempt to abstain from controversy. Topics which would be considered controversial in the eyes of the general public are discussed, but largely in terms of balancing arguments rather than indicating how the study of economics may contribute to the solution of social problems.

There is a notable tendency in some of the texts examined to stress consumer or personalized economics as a basis for introducing the subject. The concern of economics with the operation of the total economic system is thus slighted, with the result that students may gain the impression that this branch of the social studies is to be viewed as a step toward securing a job.

In the portrayal of the interpretation of economic forces as determinants of social action, the texts fall short of being reasonably adequate. The common thread running through all places emphasis on the micro-approach, showing how an individual's environment affects his choices as a consumer spending his income, his ability and willingness to work, and how the individual businessman makes his decisions. This type of treatment probably emphasizes what is most familiar to high school students, but there is a tendency in such an approach to emphasize trivialities. The books seldom use the opportunity to show how individual behavior is integrated by the market and how resources may be allocated to meet the needs of individuals and of the society. Little is done to show how the market mechanism and institutions have been modified as a result of growth in population, extension of the market, and technological development.

The power of broad market forces and the manner in which they work receive relatively little attention. The same may be said with respect to matters of public policy; the effects of governmental action are not given adequate discussion. The books describe a list of activities, institutions, concepts, problems, and policy debates, with no basic unifying theme.

Method employed in the presentation of economic topics is primarily descriptive. Analysis, as that term is used by economists, is notably lacking. The use of abstraction, such as the concept of equilibrium, is very seldom employed. Concepts of economizing and the allocation of resources are not kept constantly before the student nor used as bases

upon which to rest descriptive and institutional material. *What has been* and *what is*, tend to overshadow *why*.

All texts describe the American system of capitalism, with considerable attention being given to its institutional aspects. Emphasis here tends to be historical and descriptive, giving rather detailed attention to the multitude of industries, occupations, and transactions in our complex economy. Little attempt is made to get behind such complexities to explain how they developed.

The treatment of money and banking is another illustration of primarily descriptive treatment. The functions and types of money are discussed; services of banks are almost always listed; the use of credit, particularly by consumers, is described. Topics such as Gresham's law, fractional reserves, and measures of credit control are referred to. These and other topics relating to money and banking are given little analysis. Greater weight is given to routine operations of the monetary and banking system than to a critical view of money and credit as an important force in the whole economy.

Similar comments as to method of treatment can be made with respect to public finance, international trade, competition and monopoly, and labor-management relations. The treatment of agriculture tends to focus attention on the history and description of price support and soil bank programs. Very little attempt is made to discuss the problems of agriculture in terms of alternative use of resources, or to suggest consideration of the rule of the free market. Price theory receives some attention, but is confined to an elementary discussion of the market adjustment of demand and supply. For the most part, the discussion does not give the reader a sense of the real power of demand and supply.

Judgments relative to topics receiving disproportionate attention are influenced by individual value preferences, but some generalizations appear to be worthy of mention. The space given to consumer economics is out of balance. The emphasis upon the individual as a buyer or worker is so great as to suggest that the role of the individual as a citizen of the whole community is overshadowed. The attention given to the consumer as an individual even goes so far as giving "good advice" with respect to his actions. Disproportionate attention also appears to be given to the past as compared to the forces making for economic growth.

The standard of accuracy is generally satisfactory with respect to the use of numbers. Other types of facts, however, are handled less well, although students are not exposed to distorted factual information concerning the economy. When statements involve content which

merges with the problems of evaluating accuracy of analysis, judgment on the part of reviewers is inherently difficult. There are, however, too many cases in which theoretical concepts are presented in misleading terms, and a list of such statements would be sufficiently long to disturb a professional economist. The line between simplification and oversimplification, so disturbing to the economist, is difficult to draw. Most economic problems faced by modern society require careful analysis, however, and to gloss them over is a disservice.

Most high school texts consider the topics traditionally included in economics. Without question, however, the treatment of macroeconomics is so inadequate as to be classed as a significant omission. Other topics given such limited attention as to be classed as omissions would include non-price competition, marginal and average cost, relation of long and short run, state and local finance, product differentiation, population growth, role of technological change, urbanization, economic magnitudes in general, and the concept of welfare.

Social Problems Texts

A liberal interpretation of what is economic content would lead to the conclusion that texts in this area of the social studies deal with problems that have substantive economic content. The selection of such topics by the authors appears to attempt to serve several purposes. First, there is the common desire to call the attention of the student to the problems of real importance in the society. Another important purpose appears to be that of exposing the student to the problem-solving process as an educational technique. He is to be motivated by inquiring into problems most often encountered by students; he is to relate his study to real life situations. Most authors also stress the responsibility of each citizen to form opinions concerning problems which directly affect him and about critical issues facing the nation. In addition, some problems are treated as moral issues; students have a moral obligation to take a stand on such issues as conservation and social security.

In the light of such objectives, it is surprising to note the slight attention given to such topics as international trade, business cycles, money and banking, comparative economic systems, and economic growth. Extended coverage, however, is given to conservation, social security, housing, and health and medical care. Even with topics which might deserve extended treatment, matters of such marginal significance as qualifications for membership on the National Labor Relations Board, bimetallism, and legislative acts are often stressed.

To a very considerable extent, the subject matter is treated in a descriptive manner, and there is a tendency to catalogue facts with

little preparation for understanding them. Legislative acts are described with little indication of their purposes. Institutional detail is presented with scanty interpretation of its relationship to the economic process. Description is necessary in dealing with a functional view of the society, but too often these books seem to assume that description is an end in itself.

Some attempt at economic analysis is made, but in the majority of cases it takes the form of developing concepts—some of them meaningless from the standpoint of modern economic theory—and then dropping them. Some of the economic analysis ends at the definitional level, and there is a lack of continuity in much of it. There are numerous examples of unsupported generalizations and conclusions.

United States History Texts

In the typical public school curriculum, the high school course in United States history is a vehicle for conveying economic understanding. It is a course taken by almost all students and undoubtedly the economic education of many high school students is influenced by the discussion of economic matters in United States history texts.

Each of the texts examined recognizes the importance of economic forces in explaining the historical development of this country. All, for example, recognize persistent problems such as those of agriculture, big business, and labor. The relation of such topics to economic forces, however, is not adequately presented, since the general tendency is to present economic topics descriptively rather than analytically.

Frequently, economic facts and events are merely catalogued in appropriate chronological niches. Attention is focused upon the political manifestations of economic forces rather than upon economic relationships. Rarely are economic forces analyzed as determinants of social action or as a basis for illustrating the role of a market economy and the price mechanism in allocating resources.

The importance which is attached to various topics shifts according to the time period in which the topic is discussed, with the result that some economic topics are given disproportionate attention. The most striking lack of balance in the presentation of economic forces is attributable to the relative neglect of economic topics during the past quarter of a century. Prior to 1900, considerable attention is directed toward banking, currency, and the tariff. Since 1940, these areas are largely ignored, and none of the texts examined adequately presents the economic aspects of United States history as they are developing today.

The range of economic topics presented in history texts is almost breath taking. For the most part the treatment is chronological, being

geared to the listing of the occurrence of economic phenomena and matters of public policy in their appropriate historical sequence.

There is no reason why the selection of economic topics and the manner in which they are treated in history texts should correspond to the relative importance of such topics within the framework of economics. Exploring the structure and operation of the economy and showing the course of the nation's history are different tasks. Measured in terms of the content of economics as viewed by economists, however, the balance of economic topics and their emphasis leaves much to be desired. Important topics are omitted and others are given disproportionate attention. Excessive treatment is given to topics which may have had political and economic significance in an earlier period but which have little significance for the current operation of the economy.

The most striking lack of balance is seen in the relative neglect of economic matters since the period of the New Deal. Topics which are of major importance within the total body of economic knowledge are omitted, and topics which are discussed in detail prior to the New Deal and largely ignored thereafter became distorted. The treatment of agriculture, for example, gives disproportionate emphasis to marketing problems as compared with the mechanization of agriculture and its economic effects. Discussions of public finance emphasize the magnitude of taxes and expenditures rather than the nature and role of fiscal policy. Government regulation is given disproportionate attention as compared with governmental action aimed at stability. Heavy emphasis is given to currency and banking prior to 1900, whereas the Federal Reserve System and the nature and role of monetary policy are given meager treatment.

The subject matter of United States history, as indicated by the number of economic topics presented in textbooks, provides a valuable springboard for contributing to economic understanding. Mastery of these topics, however, does not imply an understanding of the structure and operation of the economy. This can be achieved only if historical events are related to such economic processes as competition, resource allocation, the business cycle, and economic growth.

Conclusion

The foregoing summary of the views of economists with respect to social studies texts deals with only one phase of the large problem of increasing economic understanding at the high school level. Underlying the whole project has been the conviction that the majority of economists working at the level of higher education, or in professional capacities in business and government, do not know, and have shown little concern with, the nature of text materials widely used in the high

school social studies. This report is an attempt to provide such information.

The reviewers of the texts have not presumed to be specialists in secondary education, nor have they proceeded on the basis that the social studies curriculum in its present form is worthless and should be turned over to economists as the custodians of the "real gospel." The work proceeded with a sincere realization that the goal of greater economic literacy can be achieved only through the combined and sustained effort of all who possess competence and interest in training high school students.

THIS IS ECONOMICS

By Howard S. Ellis
University of California, Berkeley

Notwithstanding the title with which I have been so generously endowed on the present occasion, I am not called upon to tell this audience what economics is. What I am supposed to do, rather, is to set forth "What Every Young Man Ought to Know" about economics as a graduating senior from an American high school. I am not aware of any peculiar qualification on my part for this undertaking, save perhaps that I was administered a course in economics in a high school a number of decades ago and somehow survived the ordeal.

There are those who believe that high school students cannot master enough of the substance of economics to warrant plaguing their souls and distracting them from the three R's. While I am a fundamentalist concerning secondary education, I reject this view. High school students have successfully coped with courses in chemistry, physics, and mathematics—to say nothing of *ratio obliqua* in Book IV of the *Gallic Wars* —which are as difficult as the basic concepts and propositions of elementary economics. As for what is fundamental, the main economic facts of life would seem as important and as interesting as the biological or physical facts for the future citizen completing his secondary education. And the three R's need not suffer much from the introduction of social science in high school curricula which at present boast such embellishments as courses in semantics, stage lighting, and creative listening.

Serious doubt on the other hand would attach to the offering of instruction in economics by high school teachers whose own college or university training included no such study; and there would seem to be no question but that the present supply of teachers qualified in economics would not support a widespread introduction of the subject into the high schools. Even this reflection does not absolve professional economists of the responsibility of indicating what the high school course ought to be. Which teachers are to be regarded as qualified depends in part certainly on what and how much economics should be offered. I therefore believe the question posed for this paper must be answered first, regardless of the supply of teachers. Schools of education and framers of college curricula, as well as school principals and school boards, will then be able to find out what they should require of prospective teachers of economics. It seems that a college major in

economics would be no less logically implied for teachers of high school economics than a major in chemistry would be implied for a chemistry teacher. At the very least, a minor in the subject is really requisite. An adequate textbook—contrary to widespread belief—is not enough.

It seems sometimes to be thought that the bottleneck of available teachers as well as the problem of opportunity costs in the high school curriculum from the student's angle would be solved, or greatly mitigated, by dealing with economic problems as illustrative material for or as an adjunct to the history courses. To be able to carry this through successfully, however, would seem to require that the teacher have first not only mastered a course in economic principles but also in economic history. It would be extremely dangerous to turn the uninitiated person loose upon European or United States history, for example, to make his own applications of such economics as he might have learned. By contraries, the high school teacher of history needs some college education in economics, since it is inconceivable that a course in United States history should not touch upon a number of economic problems, such as protective tariffs, free silver, the populace movement, the depression of the thirties, the New Deal, etc. It would be vastly superior to no economics at all to have an adroit use of economic examples in the history courses. But it would be rather doubtful whether "economic examples" can be made clear without some theory. By the time the theory and the application to policy are developed, we would seem to have too much to deal with merely as an adjunct to history.

In what follows I have been led by these considerations to try to envisage what could be conveyed to high school students by teachers who have themselves some acquaintance with economics at the college or university level; and I have supposed that we are dealing with a one-term course in economics, or a year's course devoted to the social sciences in some combination or other. The self-imposed limit of one term or semester for economics serves the purpose, in addition to its presumable realism in the perspective of the limits of the high school curriculum, of preventing the level of analysis from becoming more than the traffic will bear.

The orientation of the ensuing outline of high school economics is frankly theoretical and not institutional. Of course, some description of institutions is probably unavoidable, but this is not the primary responsibility. The aim of the instruction should be to equip future citizens with a useful apparatus of thought with which they can attack the economic questions which they will encounter in everyday life—in reading newspapers, listening to political addresses, conversing with friends, pursuing their own businesses or professions. To this end some of the fine technicalities of theory might well be sacrificed in favor of

adequate coverage in the course. International trade should be included; but money costs rather than the underlying real comparative costs can sufficiently explain specialization. The banking system and money creation are indispensable parts, but it would be possible to avoid the multiple expansion process of bank credit creation simply by referring to required reserves as a control point. Some analyses of national income, saving, and investment would also seem indispensable, but the high school student might well be spared the pyrotechnics of multipliers and accelerators.

With these preliminary observations, I proceed to indicate how a kind of minimum working knowledge of economics might be developed under four main headings; but there is no magic in either the exact sequence, number, or detailed composition of these subjects. In conclusion I will have a word to say on the greater importance of the spirit of the undertaking—of the way things are done.

I. *The Allocation of Economic Resources*

Despite the existence of affluent societies, the logical beginning of economic analyses would appear to be the old problem of the allocation of scarce resources to wants which seem always to be larger than can be fully satisfied. I should think that consumers' choice might lead off and that a good deal of pains should be devoted to making clear the concept of margins, which is, after all, the most important advantage which economics has over other social sciences. One could set forth the scheme of an economy governed solely by consumer choice, its pros and cons (as against some other kind of economy), as a first step in learning about our own surroundings by putting ourselves into a different world.

Marginal choice might then be exhibited in the producers' economy by means of the idea of opportunity costs, diminishing returns, production functions, and the influence of factor costs on cost schedules. For high school students I would, if forced to choose because of time, prefer to omit U-shaped cost functions and the intricacies of firms versus industries, in order to make very clear the difference between economic operation and planning and a purely engineering, physical, or ideological approach to problems such as highways, urban redevelopment, etc. But the idea of a supply schedule, to be used in conjunction with the demand schedule, is indispensable. Indeed, in all subsequent sections, supply and demand schedules should do yeoman's service in the exposition.

It would obviously be dangerous to go into production economics at all unless the main distinguishing characteristics of competition, monopoly, and mixed or intermediary forms were set out. Product differentiation certainly has to be introduced; this and the competitive and

monopolistic cases involve some minimum of quantification. Assuming that most students, as least those who are in the slightest degree intellectual, will probably swallow a dose of Marxian economics at some time or other, it might be wise to make brief reference to the economies of scale and tendencies to concentration and their limits. Presumably, also, the treatment of costs will also have referred to what it means "to produce" in non-Marxian economics.

Is it too much to hope that, with some experience, an intelligent teacher (this is not a pleonasm) could treat the subjects mentioned under Section I, meeting a class five days a week in the space of a month or so?

II. *The Flow of Income, Fluctuations, and Growth*

Although current generations of students have had about as much personal experience with depressions as they have of dodoes, horses and buggies, or oil lamps, they do have a feel and curiosity about economic life in primitive economies, or about Soviet or Japanese efficiency compared to our own. Beginning with a barter economy, they may be invited to envisage more and less division of labor, the interdependence of various lines of production, and finally the flow of output as a whole. Real and money income can be distinguished; and the total income of the society (as GNP probably, since this has almost become a newspaper concept) can be viewed as expenditure and value-added; but the gamut of net national income, personal disposable income, etc., would be too much.

It has never been difficult in my teaching experience to convey the idea that interruptions in the goods-money-goods sequence or injections of money can contract or expand money incomes. If the discourse is conducted in terms of putting money into the stream by bank lending or by individual's increasing his spending, the frightening distincton between *ex post* and *ex ante* can be postponed to the Greek calends for the student whose formal education stops with the high school. He may be able to lead a reasonably satisfactory life anyway. The critical role of hoarding and dishoarding and of saving and investment in the flow of income could still be made clear to him.

But I am sure that no economist would feel that he had done his duty until he had gone somewhat below the superficialities of money flows into more basic factors of economic fluctuations; and he would certainly want to have something to say as to the remedies for unemployment, overly full employment, boom and bust. Probably the ideas of sectoral overexpansion and the playing out of demand in particular lines will not prove too difficult, but it is problematic as to how far the analysis of real causes can be carried. On the other hand, I should not

feel pessimistic about the possibility of explaining the right lines of action regarding the supply of money and credit nor about the main principles of tax-expenditure adjustment for contra-cyclical operation.

From many angles it would be desirable to emphasize the importance of growth for the economy, and the idea should not be too formidable. The student might well be shown how a particular sector—as, for example, labor—has more to gain after a certain point from the increase of total social product than from an increased share in a given total. Indeed, the higher the growth rate the less the likelihood of clashing claims of various sectors. In the external affairs of the nation, growth of national product will seem to most students to be necessary as a demonstration of the viability of a capitalist system as well as for the requirements of defense and foreign aid.

III. *Distribution of Wealth and Income; Economic Systems*

The professional economist scarcely hears the term "marginal productivity" without having called up a great array of situations in which chance, fraud, coersion, monopoly, monopsony, tradition, and ignorance interfere with the conformity of actual rates of return with marginal product. If anything, he may—from a fear of being thought naïve—be inclined to overstate the limitations upon the marginal principle.

But with the layman and particularly with young people and youngsters, the opposite probably holds: they are apt to believe that profits and indeed most incomes are just matters of luck, or of playing an aleatory game; and this belief may predispose them toward taking a rather cynical or even a predatory view of wealth and income distribution. However that may be, the uninitiated person in economics is very probably not aware of any law or principle, rhyme or reason, in incomes. It would therefore be well to have a section in the high school course which recurred to the earlier section on production costs and factor proportion in order to demonstrate how the payment of income could rest upon productive contribution, upon impersonal forces, upon a market price system. Outside economics, this is an unfamiliar concept.

Naturally any brief description of a competitive private enterprise system should be coupled with the modifications introduced by monopoly; and the asperities of income distribution, even when it conforms strictly to productivity, will, of course, not be neglected. Nevertheless, a main undertaking would be to set forth the attractiveness, from many angles, of a really competitive income system and of an impersonal distributive system. It could be pointed out that an idealized sort of socialist system (such as F. M. Taylor and Oscar Lange and others have constructed) could operate upon this basis. The possible gains and the possible or probable risks and losses (including the loss

of liberty) involved in nonmarket or authoritarian systems would then follow.

It would seem natural in this general context to treat monopoly as an income, antitrust, and public utility control problem. Also in this context one would expect some analysis of labor unions and industrial relations, with explanations of terms which crop up in newspapers and other popular discussions, such as boycotts, closed shop, union shop, lockouts, industrial and craft unions, etc. I do not think the course should avoid stating the dangers of unionism such as featherbedding, the maintenance of make-work devices, and the engendering of cost-push inflation; and it should, of course, state the usual defenses of unions as welfare and bargaining agencies.

The section dealing with distribution would be a logical point at which to introduce the modifications of market incomes produced by taxes and expenditures. Some of the commoner concepts of public finance would be explained, such as direct and indirect taxes, progressive and regressive taxation, incidence, etc. Since the layman's view of taxes is apt to be that of unmitigated disaster, it would be well to represent taxes as alternative ways of consumption or investment which are settled by the voting process instead of through the exercise of dollar suffrage upon the markets. Having scored this point, however, the instructor might well wish to say some wise words about good and bad taxes, good and bad tax administration, and the good and bad uses of public funds.

IV. *International Trade and International Economic Relations*

In some quarters it is thought that international economics should not turn up in a high school course. Even in university elementary courses, where it is ordinarily admitted as no less important than other parts of economics, it often appears in a rather attenuated form because it comes last chronologically and thus becomes a residual claimant upon time. In my judgment, however, any skimping of international economics represents a bad strategic blunder, not only from the angle of intrinsic importance, but also from the angle of exploiting students' spontaneous interests.

Having rallied to the defense of orthodoxy in insisting upon the inclusion of foreign trade and finance, I may perhaps venture the heterodox view that much—perhaps most—of the important points which should be scored in this section of the course could be made without benefit of comparative real costs and the usual balance-of-payments apparatus. Both of these, in my experience—which is not altogether short—are extraordinarily frightening to beginning students. Is there

not a way of making do in high schools (I do not say in colleges and universities, of course) without them?

Certainly the basic idea to be driven home is the similarity of gains from domestic and foreign trade. Much can be done in stating the case for free trade, and in refuting the cruder protectionist arguments, by arguing from domestic trade, where no mystery and no misgivings are involved, to the *terra incognita* of international relations. The case can be set out satisfactorily (if without the final logical clinching) on the basis of money costs, and without necessary appeal to the balance of payments.

The same is, I believe, true even of the discussion of exchange rates. Some general reference to the total demand for and total supply of foreign exchange is unavoidable, and there is no reason for avoiding reference to the balance of payments in this aggregate sense. But details of the balance could be omitted in most contexts; thus sparing the beginner the worry about capital "exports" being entered on the import side, about goods consumed by Americans abroad being imports, and about the fact that a decrease of foreign-owned bank balances in the United States in consequence of U.S. exports is entered on the same side of the balance sheet as an increase of foreign-owned balances in consequence of U.S. imports.

Personally (and everything in this paper is inevitably personal), I would settle for less of the apparatus of international trade theory and more of the range of problems. Thus I do not see how one can avoid a discussion of major objectives, such as full employment, or insulation of domestic markets from foreign fluctuations, or national self-sufficiency, or development versus (or through) present gains. There must be some discussion of exchange controls, and here these conflicting goals are or should be made apparent; and in the same degree this is true of commercial policy and problems of foreign loans and aid.

Conclusion

So much for the attempt, which is bound to be somewhat subjective and arbitrary and therefore somewhat frustrating, to indicate what by all means should be included in an elementary course designed for high schools and what might possibly be spared. Selection and pruning there must be, but I would expect to find some dispersion of judgments on each point I have made.

I would expect a very considerable degree of agreement, however, on the range of matters into which I now venture: the "spirit of the thing." In the first place, I feel certain that all professional economists would earnestly hope that high school students would learn from this course

some respect for attempts to think in an objective and nonpartisan way about issues which are often decided on the basis of selfish or parochial interests or inherited prejudices. In other words, such a course ought to instill a respect for facts, and it should cultivate an attitude of intellectual fairness.

Second, economics tries—and students should be induced to feel this fact—to deal with the maximizing of the welfare of the individual as a member of society. Its ultimate aim is "doing good," but it aims to do so in a hardheaded way, without mawkishness and display of sentimentalism.

Third—and perhaps this should be both first and last in any such list—economics attempts to equip its followers with an analytical apparatus. Long after the facts, institutions, and even many of the specific theories have lapsed into the oblivion of the student's past years, he should retain a useful kit of tools. He should have some inkling as to how to set about studying and analyzing an economic problem.

Fourth, since thought cannot proceed on such matters—or at least not proceed very far or very profitably—in a vacuum, the student should be left with a few important facts and with a sense of how to go about looking for relevant facts.

Finally, according to the very concept of economics, its students should be prepared to point out that one good usually involves the sacrifice of another as an opportunity cost. We cannot have all we want of all things. Economics is thus inevitably a bit dismal: it is forever pointing out impossibilities. But of course this also points out the positive side: we are basically interested in what is possible, and economists are thus certainly not fatalists. Their message should be inspiring; it should encourage the high school student and hence the future citizen to try to make the economic world, not only for himself but also for his fellows, a better place to live in.

ECONOMICS IN THE HIGH SCHOOLS:
THE RESPONSIBILITY OF THE PROFESSION

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I

Last September about ten million youngsters entered high schools all over the United States. Of this total, as best we can estimate now, only about 5 per cent will take during their high school careers as much as a one-semester course in economics. Of the ten million, perhaps 40 per cent will go on to college, where perhaps a quarter of them will take as much as one course in economics. Allowing for some duplication, the conclusion seems inescapable that only around 10 to 15 per cent of all of today's high school students, who are tomorrow's citizens and voters, will ever take as much as a one-semester course in economics, of whatever quality.

On their way through high school, nearly all students take a course in American history. This is the one place we can be reasonably sure that every student hears something about economics—at least about economic institutions. But, as far as we can find out, this is a promise that is honored primarily in the breach. Although a few comments on the development of economic institutions and governmental policies are tucked into the chronology of the course, these are few in number, often trivial in importance, and usually superficially treated. There is no pretense at analysis of economic problems, even when they loom large in the perspective of history—for example, the Great Depression of the thirties—although we can be reasonably sure the student will hear of bimentallism and the Granger Acts.

Perhaps half of all high school students at some point take a course in "Problems of Democracy," or "Civics," or some such title, in which there may be substantial units on economics. These units are usually centered around particular problems, such as resources, security, and the like. Here again the coverage is generally descriptive and nonanalytical. Much space is given to description of laws and governmental measures, often with little differentiation between the trivial and the important. Sometimes some economic concepts are introduced: for example, place utility, time utility, and the like. But these are generally fresh out of the texts of the early 1900's and, not surprisingly, they are seldom used in any way—except perhaps to bore the students and to destroy any potential interest they may have had in economics.

Who teaches economics in the high schools, either in economics courses or in others such as problems in democracy or American history? The answer is, mainly historians, but just about everybody to some extent. Except in a few large city school systems, certainly not economists. While the facts are far from clear, it seems almost certain that a majority of social studies teachers have never had a single formal course in economics. Indeed, apparently as many as a third of those teaching actual courses in economics have not had a college course in economics, of whatever quality. Only eight states now require a course in economics for teachers' certification in the social studies. Problems in democracy courses and economics courses are not infrequently given to the coach to teach in smaller schools. As Professor Olson has indicated, the textbooks used, while sometimes of good quality, generally do little to help matters. If we recognize further that the single course in economics taken in college by those teachers who have had some economics is all too frequently of low quality, the picture seems dismal indeed.

The blame, it is important to recognize, is not on the individual teachers who do not do a very good job of teaching economics. Most of them are sincere and conscientious, and many try hard to improve their courses by bringing in outside materials, getting students involved in junior achievement work, and the like. The real problem is that they just do not have the training to do a reasonable job and have no reasonable way of getting the required additional training.

In a few schools, notably those in the big city systems and upperclass suburban areas, some very good high school economics is apparently being taught. The same is true in a few of the leading private prep schools. But these are special cases. Little wonder at the disinclination of many college students to sign up for elective courses in economics, or at the economic boners in the letters to the editor columns.

II

The attitude of the economics profession toward economics in the high schools has generally been one of disdain and disinterest. While an appreciable number of economists have, especially over the last decade, participated actively in helping to improve high school economics, in general economists have apparently felt: (1) It is better to get beginning economics students in college who have not had high school economics, because you generally have to unteach a good deal of what they have learned in high school. (2) Economics is too difficult and too important to teach in the high schools anyhow. (3) Hence, it is better to do nothing at all with economics in the high schools. (4) The

whole business is terribly low level, and somebody else ought to do something about it if anything is to be done.

I wish to argue that this position is indefensible and, indeed, intolerable; to report some major steps now being taken in the profession to improve the teaching of economics in high schools; and to urge widespread participation in further steps to continue this improvement.

The basic case for economics in the high schools is the case for democracy itself. Democracy means government by the people. Government affairs, in very substantial part, are economic affairs. High school students of today, as the citizens of tomorrow, will work and live as part of an economic system, which they must understand at least reasonably well to function as effective citizens. For democracy to work on economic issues, the people must understand; it is not enough that the leaders alone do so.

Given the fact that only 10 or 15 per cent of today's high school students will probably take a course in economics in college, it is simply irresponsible for economists to say we should not bother to teach economics in the high schools. The real alternative is for the vast majority of citizens to study no economics at all. We merely kid ourselves when we assume that the college economics course is the solution to the problem for the foreseeable future.

Only if a little teaching of economics in the high schools must be worse than none at all can the prevailing attitude of the profession be justified. The fact that economics, in separate courses or more generally in history or problems in democracy courses, is in fact so badly taught that it may be worse than nothing at all, is no justification for our refusing to touch the high school problem. On the contrary, it makes more evident the need for professional economists to help in improving the quality of the economics that is taught in the schools.

In the last few years, the physicists, mathematicians, chemists, and biologists, to name only four major professional groups, have all mounted major offensives to improve the teaching of their professional areas in the high schools. In each case, the most distinguished scholars and teachers in the profession have participated actively. If the economics profession accepts the challenge of helping to improve economics in the high schools, it will merely be following in the path of other major professions which have already beat us to the game.

One last introductory comment. While there are some 25,000 high schools in the country, a majority of all high school students are encompassed in a relatively small number of major city and large suburban school systems. Thus it is possible to have a major impact on high school teaching by influencing a relatively small number of key school

boards and supervisory personnel, although this must be backed up by helping teachers who are not now qualified to do a reasonable job.

III

The responsibility of the profession to face up to improving the teaching of economics in the high schools is clear cut and large. What, concretely, can be done and who should do it?

First, some major steps are already under way, and I wish to report briefly on these to you. Over the past decade, the Joint Council on Economic Education, with a board of trustees including economists, educators, businessmen, labor leaders, and agricultural leaders, has been increasingly active in helping to improve economics in the public schools. Dozens of regional workshops each summer for high school teachers of economics have been a main channel. The Council has also provided reading lists and, less frequently, brief statements by professional economists on current economic problems of interest in the public schools. This has been helpful, but only a partial attack on a huge problem.

Over the past few years, it has become increasingly obvious to the leaders in the profession that we could not indefinitely continue our hands-off policy toward economic education in the high schools. Professor Olson has told you of the AEA's textbook study. In 1959, the Business-Education Advisory Board to the Committee on Economic Development (on which several well-known economists serve) proposed to the CED that it take leadership in arranging a National Task Force on Economic Education, composed of distinguished economists. The CED accepted this recommendation enthusiastically and offered to provide funds for such a National Task Force if the President of the American Economic Association would appoint the members. The CED also offered to finance publication of the report and its widespread distribution to high schools and to those responsible for high school education.

After consultation with the Executive Committee, President Schultz of the AEA appointed a National Task Force in early 1960, consisting of Lester Chandler, of Princeton, R. A. Gordon, of California, Ben Lewis, of Oberlin, Paul Samuelson, of M.I.T., and myself as chairman, with M. L. Frankel, of the Joint Council of Economic Education, and Arno Bellack, of Teachers' College at Columbia, as Consultant Members from the field of secondary education. We asked Floyd Bond, of the CED, to serve as Executive Secretary. We promised to devote a year to the undertaking and to produce at the end of the year a statement of the problem as we see it, a statement of the minimum level of economics essential to good citizenship and reasonably attainable in

the high schools (if indeed this appears feasible), and a set of recommendations for further action. The National Task Force is hard at work.

I want to emphasize that the Task Force is a completely independent unit, now that it has been established. It is responsible neither to the Committee for Economic Development nor to the American Economic Association for its recommendations. The commitment to finance the study and to publish the results is a firm one, regardless of our findings. We feel highly responsible as representatives of the profession, but we do not speak for the American Economic Association in our report.

At the same time, CED has undertaken a major push on economic education in the secondary schools. Beyond the work of the Task Force, the CED has provided funds to gather together and classify the huge mass of potential teaching materials in economics available to high school teachers (pamphlets, propaganda tracts, film materials, and the like). Professor Lewis Wagner, of Illinois, will devote the coming semester to this huge job of classification. Next summer, another group of economists and educators, again financed by the CED but with complete academic freedom, will undertake the job of evaluating these materials—both as to their economic content and their potential usefulness in high school teaching. The goal is to winnow out of this huge mass with which high school teachers are flooded a reasonable subset which can be seriously considered. It is important to remember that most high school teachers are not able, with their limited training in economics, to make well-informed, independent judgments on these issues. It is hoped that this classification and evaluation task will be done partly on the basis of the recommendations of the National Task Force as to what main elements of economics are most important and feasible to teach in the high schools.

Within the last few months, still another major activity has developed. "Continental Classroom"—the nationwide early morning TV program for high school teachers and others which has attained so much fame over the last three years—will probably be devoted to economics in 1962-63. Continental Classroom was an outgrowth of sputnik, and its original goal was to update high school teachers in physics, as part of the major push in the high schools to improve science teaching. The second year was devoted to chemistry, and this year mathematics and statistics are the subjects. In each case, a group of distinguished professional leaders served as an advisory board for the program, while the actual teaching has been undertaken by one or two professional leaders, working with the aid of many others. For example, over half a dozen Nobel prizewinners participated in the Continental Classroom series on chemistry last year.

The Learning Resources Institute, the operating sponsor of Continental Classroom, has asked both the American Economic Association and the National Task Force on Economic Education, as well as the Joint Council on Economic Education, to serve as co-sponsors of Continental Classroom next year. Funds, as in past years, will be obtained from industrial donors, NBC, and the Ford Foundation, although the content and operation of the program is completely divorced from control of the donors. The American Economic Association has agreed to serve as co-sponsor of the program if it is offered in 1962-63 by an economist approved by the Executive Committee of the Association and if suitable standards of academic freedom, objectivity, and presentation are met. The National Task Force has agreed to serve as the primary advisory group on content and method, to help implement the AEA's objectives.

Continental Classroom provides an extraordinary opportunity to present economics to a large proportion of the 50,000 or so social studies teachers who do the actual teaching of economics, good or bad, in the high schools. There is no alternative comparably quick and extensive channel available. In addition to high school teachers who may reasonably be expected to watch (and even to take the course for credit) on Continental Classroom, the Learning Resources Institute feels that a regular watching audience from one-half million to a million people is likely for the course in economics.

IV

Taken together, these steps amount to a major assault on the problem of economic illiteracy in the high schools. I should like to conclude with two comments. One is on the crucial question whether there is a central core of economics that can reasonably be taught to high school students. The second is what, if anything, can you as members of the profession do to help?

We on the National Task Force believe that economics can be taught in the high schools, that enough economics can be taught to improve significantly the understanding of potential citizens, and that economics should be taught in the high schools. Moreover, we are prepared to state by next summer what we believe is a reasonable set of economic concepts and institutions and a reasonable level of proficiency in using these in economic analysis for teachers of economics in high schools.

It has been striking to all of us that we have come to substantial agreement so quickly on the fundamentals. While we shall not have a formal report until summer, I am divulging no great secrets when I say that we agree that the first, and most essential, step is to get students

to see that here is an orderly, objective way of thinking about economic problems, in contrast to recourse to emotional, snap judgments. Second, we believe that we can specify a reasonably brief list of economic concepts and institutions to which students ought definitely to be exposed. Third, we believe we can suggest, as examples, some major problem areas and some reasonably simple ways of getting students to look at these problem areas, using an orderly evaluation of alternatives and some simple economic concepts in doing so. We are agreed that the great bulk of the material in the traditional college elementary course must be foregone. Emphasis on a way of thinking and a very simple but fundamental set of analytical tools, focusing on markets and the price system as an allocative device and on aggregate demand and aggregate supply in understanding macro problems, is what we have in mind. We expect to have our heads bloodied by both our professional colleagues and high school educators when we finish. But we think the job is so important we are willing to take the chance. The hardest blows of all may well come from the pressure and special interests groups, most of whom apparently believe that high school economics should mainly be devoted to explaining why their special interest is the one that should prevail.

Lastly, assuming that I have convinced you that the profession has a large responsibility in this connection, what can you do to help?

First, you can speak out in your communities that economics is important and that it can usefully be taught in the high schools. If such talk is to have any impact, you may as well recognize at the outset that you will have to give up most of the technical apparatus and technical jargon to which we are accustomed in our inner circles. Indifference maps, the niceties of marginal cost and marginal revenue, and the like have their places—but not in high school economics courses or in conversation with the people who control what economics will be taught in the high schools. If their inclusion is the test of a useful high school exposure to economics, we might as well give up now. But we in the Task Force do not believe this is the proper test.

Second, you can support Continental Classroom, both by helping to call it to the attention of leaders in your communities and by backing up the approach to economic analysis which will be involved if it is given. You can be sure that this approach will emphasize an objective careful way of thinking about economic problems, the development of a few simple economic tools, awareness of major problem areas, and careful attention to presentation of competing values on public policy issues. Continental Classroom will not be for or against some particular brand of economics or some particular interest group in society. Many watch-

ers will be very much surprised at what they see on Continental Class-room. The fact is that most people still think of economics as a body of pronouncements for or against particular points of view.

Third, insofar as you think they are reasonable, you can help in bringing attention to the recommendations of the National Task Force on Economic Education. And you can help explain them and back them up when, as the experts anticipate, they are widely discussed by school boards and school administrators in local communities throughout the land during the next academic year.

Fourth, many of you can help at first hand the teachers and school administrators in your communities. School boards, school administra-tors, and teachers themselves are generally sincere, hard-working peo-ple, anxious to introduce better economics into the schools. Their big-gest problem is they just do not know how to do it effectively. With the aids coming forth over the next year or two, we hope there will be a basis for moving forward. If asked, you can help. If not asked, you can volunteer. But never forget, the problem of teaching economics in the high schools is very different from that of teaching it in college, and certainly than in the graduate schools. We of the National Task Force have learned a lot and have had to think a lot about what is really important in economics for the citizen. The same kind of experi-ence may have some valuable results for you, too, as well as helping to push up the level of nationwide economic understanding. It is doubt-ful that any other educational goal is more important for us as econ-omists.

DISCUSSION

SHOREY PETERSON: The program of this session, as I understand it, asks our Association to recognize and get behind what—hopefully—is a major movement. My remarks will center on the movement and the conditions of its success; and only incidentally on these papers which are part of it.

To one who believes there is broad need for education in economics, Professor Bach's figures on its meager extent are discouraging. We should expect an opposite condition in a society in which both personal economic endeavor and public economic problems are so obtrusive. Effort to spread such education is not new and we must infer, therefore, that substantial barriers stand in our way in this present effort. Thus, while we should help with the substance of instruction, as the Task Force of economists and Professor Ellis in his paper are doing, we should view the removal of these barriers as our main task.

We are confronted, it seems, with something akin to the "take-off" difficulty that holds backward countries in bondage. On the one hand, meager teaching of economics makes training of teachers seem unimportant and excuses the reluctance of education students to study a subject they find difficult. On the other hand, social studies teachers untrained in economics have little interest in teaching it and school managements wisely do not ask them to or do not require students to take what little they offer.

Some outside push is needed, and professional economists, it seems, should provide it. But here a second barrier appears: in the indifference of the profession, as Professor Bach sees it, or in their doubt whether what can be done is really worth doing.

A third barrier may lie in lukewarm, or negative, public attitudes. If economics appears, as it often seems to, either as a lot of abstractions or as a miscellany of unrooted and controversial opinions, there can be little public demand to push school authorities in the desired direction.

The main task of the leaders of the movement seems, then, to be a selling job, for this is the process by which these barriers must be breached. Economists quite naturally prefer to give advice on the content of teaching, and I would not divert them; but I offer this suggestion that perhaps the best selling job can be done by setting forth our product so that it seems indispensable. Training for citizenship is an attractive slogan; but economics should appear as something that will get more intimately into students' thoughts and actions than, say, by increasing their wisdom on the farm problem.

Without presuming to do more than suggest a few ideas that seem adaptable to a little window dressing, let me illustrate what I mean.

Suppose we put in our show window the simple, but basic and expansible, lesson that income, as every housewife knows, means little except as it can buy goods; that there is nothing to buy except what is produced, so that there is a meaningful national product which includes everything we can

obtain with our wages, profits, and taxes; and that each firm can divide among its workers and owners only what the productivity of its labor and capital provides, as the market values its output. We have here, I believe, the basis of instruction of obvious significance. When, for example, a prominent radio spokesman offers his labor audience this daily injunction, "Take it easy, but take it," we should be able to show a real need for teaching that puts the future worker in a valid economic setting, aware of the contribution of his honest effort to the general welfare.

In this setting the young student can move easily to a related lesson which puts technological advance in proper light, as the basis of rising output and well-being but also as the source of job disturbance, which may induce sabotaging make-work practices but should lead to sounder alleviating measures. Moreover, he should at this point acquire a firm sense of what there is to divide and, with some additional facts and a little theory, approach the distribution problem without rancor. Once he understands that income payments are also costs which must be covered, he should begin to see that the division of income is not just a matter of power and greed but rests, like total income, on something more basic.

Perhaps the most necessary lesson is a meaningful picture, without much detail, of the economy as a whole. At a time when it is crucial that we see —and that the whole world sees—the essentials of our system, confusion is widespread as to what these essentials are and as to where we stand now in relation to our traditional conception of them. It is very easy for manifestations of government interference and of the power of private groups to obscure what, in fact, they merely modify, or even buttress. Accordingly, it seems urgent that all students achieve a secure, if nontechnical, grasp of these two fundamentals of our system, that we depend (1) on voluntary private enterprise to get things done and (2) on markets, and on the value relationships operating in them, to achieve order and efficiency—to make sense among the incredible array of wants, resources, and techniques that, in the absence of effective control, would mean chaos. They should see, moreover, that our principal freedoms as producers and as consumers—and I would add other freedoms—are rooted in these arrangements. And, of course, it is only with this structure in mind that the student can have perspective of all the particular problems and policies he encounters—a perspective more important, I believe, than his probable grasp of specific issues.

Very tentatively, I am sketching a minimal nucleus of instruction that may be usable in various ways in various courses, in contrast to the fuller offering Professor Ellis has described, though an appropriate part of such an offering. More positively, I am presenting a few ideas that we may be able to present to the schools and the public as indispensable in the education of everyone. There is much to be said for the larger program where it can be introduced, though perhaps not a mere junior edition of a college course; and certainly we should insist that far more than this minimum is essential in the training of teachers, even though they teach no more than this. But let us make the minimum as vital and appealing as we can, designing, to the best of our ability, a product that will sell.

E. T. WEILER: Reading these papers was a particularly heartening experience. It was gratifying that two distinguished economists had independently decided that the elements of economic reasoning are within the grasp of public school students, and that the economic profession as a group should do something about this. Many of us have been concerned with this problem for years.

Both Professors Ellis and Bach have asked for a separate course to be taught in the high schools by competent teachers who understand the spirit of the discipline as well as its techniques. Both have asked that we make theory the center of the course. Those of you who have examined the high school texts and know how heavily they emphasize consumer finance will see this as a distinct step forward. Both have also noted in their papers that the citizen is not in a position to think about public policy without a basic understanding of economics. Professor Ellis has designed his semester course around the needs of the citizen, and Dean Bach has promised that his Task Force would present a "statement of the minimum level of economics essential to good citizenship and reasonably attainable through the high schools."

I would agree that a theory oriented economics course for the citizen is a good first goal in view of the present state of economic illiteracy in the public school system. The economists in this audience who have been participating in the many workshops sponsored by the Joint Council on Economic Education will agree, I am sure, that the establishment of a first-rate course in economics in the high schools would be an important milestone.

But I have one basic objection to both papers—and that is that the goals expressed by both Professors Bach and Ellis are too modest. If our goal is to develop a high school economics course, we shall, I fear, have about as much impact on the students as citizens as we are having in our college courses—and I am afraid this is all too little.

Students in the public schools are continually learning about economics from their homes and from their own work experiences. From the first through the twelfth grades they are constantly in the process of developing some kind of economic theory. Much of it is intuitive; most of it is wrong. But the fact is that they are, through their own experience with the economic system, developing basic concepts which are likely to stay with them for many years. If we wait to teach them economics until the last year of high school, we shall probably spend most of our time undoing the errors they have developed as a result of their own learning experience as participants in the economy.

I would propose that we not limit ourselves to the development of a good economics course in the high school, commendable as it is. This would mean we would be concerned with the developing of teaching materials that could be used in each of the grades as the student becomes more aware of the economic system around him. Professor Lawrence Senesh has been experimenting in Elkhart, Indiana (a city of about 40,000), with the teaching of economics in the first twelve grades. To date he has developed material for the first two and is now working on the third grade. I am sure you would be impressed, as I have been, with the ability of these students to understand

the elements of economic reasoning, as they attempt to make sense out of what they see around them. By teaching economics through a series of special units prepared for the teachers in the elementary grades, we are able, we think, to help the student arrive at an intuitive understanding of the economic system which does not later have to be destroyed before he can learn effectively from his high school course.

I am sure that many of you will object to this on the grounds that economic theory is too complicated and too difficult to teach in the elementary grades. Taught as you and I learned economic theory, it obviously is too complicated; but, I submit, there is a method of teaching any important relationship at any level if the persons developing the unit really understand both the subject matter and the student. While it takes a high degree of inventiveness to develop the units, I am convinced that the usual public school teacher, if properly prepared, can teach the elements of economic reasoning through the entire twelve grades, not only in the twelfth grade.

Now with regard to the high school course I agree that our primary objective should be economics for citizenship; however, I do not like the implication (which perhaps neither the speakers intended) that we should therefore concentrate on the minimum amount of economics required for citizenship. I have been impressed, as I am sure many of you have been, with the remarkable things which are being done to the curriculum in mathematics. Students are not only being shown the usefulness of mathematics but they are also being given an understanding of the aesthetic qualities of mathematics. If, while this is going on, we concentrate too much on the bare citizenship requirements of economics, we are likely to leave the good students unsatisfied. We are, in fact, very likely to send an even larger percentage of our brightest and most competent students into the physical sciences and away from economics or business courses.

While we are developing a new economics course for the high schools, the mathematicians and physicists are doing the same and many of our students will have the basic mathematics necessary to do elementary linear programming problems; others will be able to read the Congressional hearings intelligently.

I think we should be very careful in the development of an economics course that we go far enough to excite the really capable student who can go on to become an economics major in our universities and later on can take his place as a leader in public discussion.

To conclude, as a first step, I think our two first speakers have done an admirable job. A new economics course in the high schools is an important milestone, but it is only a milestone in the long-time development of economic literacy of public school students. We should go further, it seems to me, both in developing economics in the elementary grades and in exciting our better students to go on to do additional work in economics.

PROCEEDINGS

OF THE

SEVENTY-THIRD

ANNUAL

MEETING

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ST. LOUIS, MISSOURI
DECEMBER 28-30, 1960

ANNUAL BUSINESS MEETING, DECEMBER 30, 1960
CHASE-PARK PLAZA HOTELS, ST. LOUIS, MISSOURI

The Seventy-third Annual Business Meeting of the American Economic Association was called to order in the Khorassan Room of the Chase-Park Plaza Hotels at 5:00 p.m. by President T. W. Schultz. The order of business followed the brief agenda prepared and distributed before the meeting. The minutes of the business meeting of December 30, 1959, were approved and the minutes of the Executive Committee meetings and the reports of officers and committees of the Association were ratified. These reports are published in the "Proceedings" and constitute official actions of the Association when approved at the annual business meeting. This was called to the attention of the members specifically in connection with the amendment to the constitution of the American Council of Learned Societies—of which the AEA is a constituent society—ratified by the Executive Committee at its March, 1960, meeting and made official by the affirmation of this action at this meeting.

The reports of the Secretary-Treasurer, the Finance Committee, and the Auditor were presented by J. W. Bell. Copies of financial statements, financial holdings, and the Auditor's Report were made available for inspection. Meeting plans for 1961 to 1964 were outlined and suggestions for time and place of future meetings were solicited. Since commitments for hotel and convention facilities are made far in advance, it is advisable for us to make selections for 1965, 1966, and 1967 before accommodations are pre-empted by others.

Major activities of the Secretary's Office were described: e.g., increased membership—in part a carryover from last year's membership promotion; the results of efforts to induce prompter payment of membership dues; the increasing use of the AEA information booklet and reprints of the *Directory-Handbook* Exhibits II and IV, "The Profession of Economist," and the mounting correspondence pertaining to career opportunities and employment outlook. The advertising policy was outlined, as was also the use of the mailing list.

The financial condition of the Association was briefly reviewed and the chief sources of income and of expenditure were explained. Although most expenses have continued to rise, some economies have been effected and regretably some of these involved postponement of directory materials. No supplement to the 1956 *Handbook* has been published since the 1957. Changes in practice in billing members after receiving only one number of the *AER* caused some confusion but this particular billing has on the whole induced prompter payment of dues. Some unexpected income from the annual meeting in Washington, interest from the investment of unemployed foundation funds, the profit on sale of securities, increases from a larger membership in dues and subscriptions and from advertising and from the sale of the use of our mailing list transformed an anticipated deficit into a wholesome net operating

income and a gratifying increase in surplus. Attention was called to the interpretation given in the Treasurer's Report, which should be read in connection with the Auditor's Report to clarify the status of foundation funds shown on the liabilities side but intermingled with investments on the assets side. The existence of contingent liabilities should be considered before new appropriations are made.

The Treasurer reiterated his conviction that members and subscribers are not paying their way, and that we may not long continue to depend upon fortuitous sources of revenue to balance the budget. He also stressed that, despite increased income from nondues sources, this year's budget would barely have been in balance without income from our investment account, which source of revenue has prevented a deficit eleven out of the past fifteen years. It is quite possible to rely upon investment gains and upon accumulated unappropriated surplus for some years to come, but this should not appeal to economists as a sound long-run policy. It would seem appropriate to plan for an increase in dues before the actual need arises.

The Treasurer described the work of the Finance Committee and again commented on the effective manner in which his colleagues, C. Wells Farnham and C. D. Anderson, have handled the investment portfolio. Under their guidance, we have been able to take substantial profits while at the same time improving materially the value of our investment holdings.

The Treasurer commented on the contents of the Auditor's Report, and called especial attention to Exhibit 3 of the report on statement of special funds. He was glad to report that the firm of David Himmelblau & Company has been asked to continue their services for the coming year.

The above reports were accepted with a vote of thanks to the members of the Finance Committee and to the Auditor.

The report of the Managing Editor, B. F. Haley, summarized the activities of the Editorial Office and of the Editorial Board. Copies of his report were distributed for inspection, and Professor Haley commented upon the number of articles and communications received, classification of the contents of the *Review*, the increased costs of printing and mailing, and our policy with respect to reprints.

Limitations of time permitted the reports from only two committee chairmen: G. L. Bach described the work of the National Task Force on Economic Education and the conditions of co-sponsoring of the Continental Classroom TV Program, and R. A. Gordon reported on the activities of the Committee on Economic Abstracts. The Committee is considering the feasibility of publishing abstracts of articles from economic journals. An experimental dummy will be prepared before a decision is made.

The reports of officers, committees, and representatives are published in the "Proceedings" as follows:

On recommendation of the Executive Committee, it was VOTED to amend our bylaws so as to change the title of "First Vice-President" to "President-elect" and to make other consistent changes where necessary to put this into effect.

Report of the Committee on Elections. The report of the Committee on Elections and the certification of the election of new officers for the year 1961 were presented by the Secretary, as follows:

In accordance with the bylaws on election procedure, I hereby certify the results of the recent balloting and present the reports of the Nominating Committee and the Committee on Elections.

The Nominating Committee, consisting of George W. Stocking, Chairman, Vincent W. Bladen, Everett E. Hagen, C. J. Hitch, Frank C. Pierson, and Edwin Young, presented to the Secretary the list of nominees for the respective offices:

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For First Vice-President
Edward S. Mason
</div>

For Vice-Presidents	*For Executive Committee*
Solomon Fabricant	Kenneth J. Arrow
Milton Friedman	Abram Bergson
Walter W. Heller	Ewan Clague
Richard A. Lester	Charles P. Kindleberger

The Committee on Elections, consisting of Herbert V. Prochnow, Chairman, Arthur R. Tebbutt, and James Washington Bell, prepared biographical sketches of the candidates and ballots were distributed early in November. The canvass of ballots was made on December 19, 1960, and the results were filed with the Secretary.

From the report of the Committee on Elections, I have the following information:

Number of envelopes without names for identification	86
Number received too late	17
Number of defective ballots	3
Number of legal ballots	3,583
Number of returns from the mail ballot	4,013

On the basis of the canvass of the votes cast, I certify that the following persons have been duly elected to the respective offices:

First Vice-President (for a term of one year)
  Edward S. Mason
Vice-Presidents (for a term of one year)
  Solomon Fabricant
  Richard A. Lester

Members of the Executive Committee (for a term of three years)
    Ewan Clague
    Charles P. Kindleberger

The report of the Committee on Resolutions was then read by Redvers
Opie:

Be it resolved that this Association express its gratitude to all who have worked to
assure the success of its Seventy-third Annual Meeting. The Association is especially
appreciative of the contributions made by the following individuals and organizations:
our President, Theodore W. Schultz, for his original and stimulating Address; First Vice-
President, Paul A. Samuelson, to whom the function of program-making was delegated
by the President, for producing a well-conceived program with broad appeal to the di-
verse interests of our large membership; the members of the Executive Committee, whose
many contributions to Association affairs included valuable assistance in originating and
implementing the program; our dedicated Secretary-Treasurer, James Washington Bell,
whose responsibilities begin with the preliminary arrangements for the meeting and end
only with the publication of the *Papers and Proceedings;* all those who presented papers
or participated in the discussion of them; Homer Jones, the thoughtful and assiduous
Chairman of the Local Arrangements Committee, W. E. Walker, the Vice-Chairman, who
willingly shouldered responsibilities on behalf of Associations of which he is not a mem-
ber, and their associates on the Committee, George W. Coleman, J. Warren Edmiston,
Jr., Werner Hochwald, Carl A. Dauten, Harry B. Kircher, Elmer P. Lotshaw, Edwin J.
Gross, Carl T. Arlt, J. P. McKenna, William H. Kester, Norman N. Bowsher, Leroy J.
Grossman, A. James Meigs, William R. Bryan, and William Abbott; the United States
Employment Service, Missouri Staff, for their generous collaboration in the operation of
the Employment Register; the St. Louis Convention Bureau for their help in general in
making arrangements for the Meeting; and the management and staff of the hotels for
the physical facilities and personal services that they provided for the comfort and to
the satisfaction of our members.

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REDVERS OPIE, *Chairman*
EUGENE CLARK
GEORGE B. KOHNEN
</div>

The meeting was adjourned at 6:00 p.m.

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JAMES WASHINGTON BELL, *Secretary*
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REPORT OF THE SECRETARY FOR THE YEAR 1960

The Executive Committee minutes are a record of official acts of the Association. Presented below are the minutes of the March and December, 1960, meetings. Following these minutes is a summary of the year's operations, with comments and interpretations concerning the Association's activities.

Minutes of the Executive Committee Meetings

1. Minutes of the spring meeting held in New York City, March 25-26, 1960:

The *second meeting* of the *1960 Executive Committee* was called to order at 9:30 a.m. at the Biltmore Hotel, New York City, March 25-26, 1960. The following were present: T. W. Schultz, presiding, G. L. Bach, J. W. Bell, A. F. Burns, L. V. Chandler, R. A. Gordon, B. F. Haley, A. L. Harris, P. A. Samuelson, and G. W. Stocking, Mabel F. Timlin, James Tobin, and R. A. Young. The absent were: M. A. Copeland and J. K. Galbraith. Attending as members of the Nominating Committee were: V. W. Bladen, E. E. Hagen, C. J. Hitch, F. C. Pierson, and Edwin Young. Attending as guests were: F. H. Knight, B. W. Lewis, J. P. Miller, H. M. Somers, and Paul Webbink.

1. *President's Remarks* (T. W. Schultz). The President outlined the order of business and procedure to be followed and called attention to selected items of particular importance. In addition to the reports of officers and of the standing committees starred on the agenda sheet, he referred to two new proposals related to the work of the Committee on Research and Publications and the Committee on Economic Education. We are not organized to assume the responsibility of undertaking professional jobs; yet we are in many respects the most logical organization to grapple with the problems presented. It was agreed that account should be made of items of overhead cost and approval of overhead expense should be agreed upon when accepting new grants. New appointments and replacements on several committees were announced.

2. *Minutes.* The minutes of the Executive Committee meetings of December 27 and 30, 1959, available in page proof, were reviewed and approved as corrected.

3. *Report of the Secretary* (J. W. Bell). The attendance, financial results, and other arrangements matters reported from the Washington meeting were reviewed. The co-chairmen of the Committee on Local Arrangements (R. T. Bowman and Ewan Clague) reported an exceptionally large registration of 5,600 persons. About 40 per cent of the total (2,800) were member of the AEA. Our share of the surplus net income will help offset the deficits anticipated in the December budget. The statement of policy, that the meetings should be self-supporting but not profit making, was reaffirmed. Plans for the 1960 meeting in St. Louis were briefly described and members were asked to submit suggestions of good prospects for local arrangements chairmen for the New York, Pittsburgh, and Boston meetings. Changes in size and composition of membership and of subscriptions were reported. The result of last summer's membership promotion was reflected in the figures and evidence to date seems to indicate that members are responding favorably to our campaign to pay promptly and help save billing costs. Administrative costs and printing and other costs of operation were reviewed.

An account of permissions to reprint was prepared but not presented in detail. A similar detailed report on the use of the mailing list privilege was submitted but not read.

It was VOTED to authorize the publication of a "who's who" directory of the AEA in 1961-62 if it proves feasible to do this. For mailing purposes, the handbook soon becomes dated and the use of the up-to-date mailing list is preferable where large numbers of names are involved.

The size and cost of the May, 1960, *Papers and Proceedings* were described and attention was called to the target set when the volume was planned. Despite all efforts, this year's volume will again be large and expensive and of the character which prompted the Committee on Association Deficit to recommend the appointment of a Committee on the *Papers and Proceedings*. No action was called for; the matter was referred to the Committee on the *Papers and Proceedings*. The 1960 revision of the information booklet has been printed (1,000 copies). These are used as convenient vehicles in membership promotion. The size of the advertising section of the March, 1960, *American Economic Review*

has been somewhat enlarged and a few advertisements are being taken for the 1960 *Papers and Proceedings*. Further progress in advertising promotion was mentioned, but a prepared detailed report was not submitted. The use of the AEA mailing list in direct mail advertising is increasing in volume. The work of the AEA counsel was discussed. Several of the officers volunteered to make discreet inquiry concerning the interest which practicing or academic lawyer-members in Washington might have in serving the Association in this capacity.

4. *Reports of the Treasurer and Finance Committee* (J. W. Bell). Reference was made to the statements of condition and results of operation found in the Treasurer's Report and the cumulative income-expense table published in the May, 1959, *Papers and Proceedings* (page 656). Operating results affecting changes in the 1960 budget figures shown in the Treasurer's Report were explained and appropriate figures indicated. Although some expenses have increased and appropriations have been made which are not reflected in the December budget figures, a surplus from the annual meeting, increased revenue from advertising and sale of mailing list will just about balance the budget as of this moment instead of showing a $3,000 deficit. The accounting of grants received from foundations was presented in broad outline and attention was called to the desirability of keeping these funds from being intermingled with our own appropriations and expenses.

The recommendation that we increase dues and on the terms described in the minutes of the business meeting and in the reports of the Secretary-Treasurer (1960 *Papers and Proceedings*, page 687, and in the 1959 *Papers and Proceedings*, pages 608-09, 625) was repeated. The matter was briefly discussed, but decision was postponed until after the Committee on the *Papers and Proceedings* had submitted its final report (see March 26 progress report referring to a final report in December, 1960).

On behalf of the Finance Committee, the Treasurer reported changes in the list of securities held, their total value cost and the market as of February 23, 1960. Separation of operating funds representing foundation grants from Association funds was taken into account.

5. *Report of the Managing Editor* (B. F. Haley). No significant developments have occurred since December in the Managing Editor's office. Attention was called to the increased number of copies and pages in the *AER* with the resulting increase in the budget figure from $42,000 to $43,375. Also, an increase in the editorial and clerical salaries from $13,100 to $14,600. A list of names of nominees to the Editorial Board to replace two members whose terms are expiring was submitted for consideration. The list was approved, with the addition of several names which might be considered as substitutes for those listed. The selection is to be made and announced at the December meeting.

6. *Reports of Standing and Special Committees.*

6a) *Committee on Research and Publications* (J. P. Miller). The announcement of J. P. Miller's retirement as Chairman of this Committee prompted suggestions that the Committee be reorganized so as to include only one representative to SSRC, one NBER, and three AEA, reducing the size from seven to five members, with Gardner Ackley as Chairman. It was VOTED to approve this proposal and the $500 Committee appropriation was continued to meet the costs of meetings and operations.

Professor Miller reported that:

(1) Volume X, on Economic Development, of the "Readings Series" may not be completed, since no progress has been reported for some time.

(2) No new items have been added to the "Translation Series."

(3) The translation of foreign classics project, for which a Ford Foundation grant of $25,000 has been received, is to be administered by the Committee on Research and Publications. Professor Miller reported that a recent proposal by W. W. Leontief to translate a significant Russian book in his field was considered, but it has been learned that the project has been accepted by an English organization.

(4) The *Committee on Surveys of Foreign Economic Research* (G. H. Hildebrand) met at Harvard University, March 5, and outlined a program of operations. The first project will consist of six essays on the general theme, "Major Issues of Theory and Public Policy Inspired by Postwar Economic Problems." Excluding countries using written English, those selected were USSR, Scandinavia, Japan, Germany, Italy, and Eastern Europe. Insofar as possible, citizens of the countries are to prepare the essays, which it is expected will be completed by June 15, 1961.

The Committee projects are to be financed by a Ford Foundation grant of $40,000. Stipends to contributors have not yet been determined but a temporary allocation of $1,500 has been made to the Chairman.

(5) The cumulative index project will probably be completed this year. The pricing problem has not yet been solved, and it appears obvious that further subsidization will be

necessary if the price is to be brought down to a mass-market level, although we can count on sales to libraries assuring a minimum-size edition. The Asia Foundation may underwrite some sales abroad. Individuals cannot be expected to pay as high a price for five-volume sets. We have nearly exhausted Foundation funds ($4,750 left) granted for the purpose and we will need to draw upon the $6,500 contingent appropriation made by the AEA. After further discussion, it was VOTED that J. P. Miller be authorized to negotiate with the Ford Foundation for additional aid in the amount of $10,000 in order to enable us to reduce the publication price substantially, with the understanding that the AEA add $2,500 to its present appropriation.

Proposal for Economic Abstracts and Data Repositories (Richard Ruggles). A matter of new business closely related to the work of the Committee was presented by Professor Ruggles; viz., (1) A Proposal for the Development of Economic Abstracts; (2) A Proposal for an Exploratory Committee on Data Respositories in Economics. With respect to the first, reference was made to groups active in producing abstracts in economics and in other fields, to the *Social Science Abstracts,* and to the exploratory efforts of the AEA and SSRC committees to develop systems of abstracting economic journals.

The President was authorized to appoint an *ad hoc* committee, consisting of R. A. Gordon, Chairman, W. H. Nicholls, Gardner Ackley (present members of the Committee on Research and Publications) and J. J. Spengler, the committee's function being to explore the feasibility of such a project after having reviewed previous experience. It was suggested that this committee meet before the SSRC meeting and that it seek the co-operation of editors of economic journals. The committee is to report back their findings to the Executive Committee.

The Executive Committee also reserved judgment on the second proposal until the dimensions of the project could be thoroughly explored. (It was suggested that Richard Ruggles might need the assistance of technically qualified personnel, working a year or more, in order to do this.) Previous committees of the AEA and the SSRC have given some attention to the problem of conserving records and other economic data. Any new proposal should benefit by past experience. The report of this exploratory committee might then be brought back to us and thence to the SSRC or a more appropriate agency if it should be so determined.

6b) *Committee on Economic Education* (B. W. Lewis). The activities of this Committee include: (1) the textbook studies directed by P. R. Olson; (2) the status of the social sciences in secondary schools; (3) the relation of the AEA to the Joint Council on Economic Education and the CED; (4) the registration of economists in economic education; and (5) educational conferences. Developments since December were described with respect to the first three items, but special attention was devoted to a proposal from CED that the AEA set up a task force in economic education. After a thorough discussion of the implications of this proposal, it was VOTED to authorize the Committee on Economic Education to explore and to develop a project providing for the creation of a task force on economic education in secondary schools (1) to provide a frame of reference for high school teachers, administrators, and curriculum specialists which clarifies the meaning, nature, content, and methods of economics (at least that portion of economics which should be thought of as the minimum to be understood by all high school graduates) and thus to provide somewhat greater direction for the economic education movement and (2) to set forth recommendations for a continuing program designed to provide better teaching materials on economics for the public schools. The task force is to be made up of economists of high national reputation. The Committee on Economic Education and the President of the Association are to establish and appoint a group of competent economists and educators to constitute one task force and to receive funds for this purpose from CED and from other sources to carry out the foregoing project.

Permission was granted the Committee to act as joint sponsor with the Joint Council on Economic Education of meetings of educators and economists at national, regional, and state levels arranged by JCEE, with prior approval of programs by the AEA Committee and with a representative of the Committee present.

It was VOTED to authorize the appointment of two delegates to the San Diego meeting of the National Commission on Teacher Education and Professional Standards, the expense money to be drawn from the appropriation available to the Committee.

6c) *Committee on Association Deficit* (S. E. Leland). The following excerpts from the Chairman's letter of January 16 indicate the status of the Committee's activities: "Your Committee on Association Deficit does not have a report to make at this time. We think that the recommendations contained in our last report solved the current situation. To repeat the conclusion arrived at by the Committee that, if the Association should contemplate an increase in dues, the action should be taken long enough in the future so

that everyone can be advised fully about the impending raise in dues before it becomes effective. The Committee thought this highly essential if a decrease in membership occasioned by increasing the dues is to be avoided. The Association is to be congratulated on its good showing for last year."

6d) *Committee on the Papers and Proceedings* (Roy Blough). A brief report was presented, dealing with the inquiries of the Committee concerning the content and size of the 1960 *Papers and Proceedings* and the Committee's plans to survey members' opinions of the *Papers and Proceedings* relative to costs of producing the volume. The question of increased dues is also a concern of the Committee.

6e) *Committee on Successorship, Secretary-Treasurer* (A. F. Burns). No report.

6f) *Committee on Carnegie Travel Grants* (L. G. Reynolds). Three awards have been made so far this year. No successor to Solomon Fabricant appointed.

6g) *Committee on Academic Freedom and Civil Liberties*. No report.

6h) *Committee on Professional Ethics* (R. D. Calkins). No report.

6i) *Census Advisory Committee* (Solomon Fabricant). The panel of advisers has been named and the Committee is actively co-operating with the Bureau of the Census.

6j) *Committee on Honors and Awards* (Clair Wilcox). No report submitted. The Secretary was instructed to send copies of the report of the Committee on Additional Awards to Younger Economists to the Chairman, Clair Wilcox.

6k) *Committee on Additional Awards to Younger Economists* (H. M. Somers, acting for M. A. Copeland). An abbreviated report was submitted in page proof. The full report, including appendices, was distributed for inspection. After full discussion and numerous preliminary motions, it was VOTED that the report of the Committee be received and that the Committee be discharged, with thanks for yeoman service performed; that the Committee's recommendations be not accepted; and that the report be transmitted to the Committee on Honors and Awards with the information that the recommendations have not been accepted.

6l) *Nominating Committee* (G. W. Stocking). On the evening of March 25, the Executive and Nominating Committees met as an electoral college to consider nominees for the office of First Vice-President (1961). After a full discussion of the list of names considered by the Nominating Committee, a vote was taken on the two top nai. ^s preferred by all voting members and after the ballot count, the consent of the nominee selected was obtained. Nominations for Vice-Presidents and Executive Committee members were also discussed. The vacancies on our roster of foreign honorary members had been referred to this Committee at the December meetings. Names of suggested nominees received from various sources to fill present vacancies were discussed and two new names were suggested. It was VOTED that no one be elected at this time. The Nominating Committee expressed their feeling of inadequacy in making selections and suggested that the old Committee on Foreign Honorary Members be re-established. The Executive Committee took no action on this suggestion but VOTED that no honorary members be selected at this time.

7. *Miscellaneous Reports.*

7a) *International Economic Association* (Gottfried Haberler). A mimeographed copy of a report prepared by E. A. G. Robinson was distributed. The report contained a detailed account of the IEA activities and future plans. There was no discussion, but the continuation of our $400 payment of dues for 1960 was confirmed.

7b) *Institute of International Education* (Theodore Morgan). No report was submitted, but reference was made to the activities of the Economics Institute for Foreign Students and the 1960 summer session program at the University of Colorado.

7c) *Asia Foundation Grant.* Several Asian economists in this country attended the annual meetings at Washington with the aid of travel subsidies. Through the courtesy of the Asia Foundation, complimentary or half-price memberships for one year had been offered to some four hundred graduate students in American colleges and universities, with the result that about a hundred complimentary memberships and a few subscriptions are charged to this account. Members wishing to dispose of back files of the *AER* have found placement for them in foreign libraries with the aid of the Asia Foundation.

7d) *Foreign Honorary Members.* See 6l.

8. *Reports of Representatives.*

8a) *ACLS* (F. H. Knight). Professor Knight brought us up to date on ACLS activities since December and referred to his report published in the "Proceedings." To make the record clear, it was VOTED to ratify our approval of the amendment to the constitution of the ACLS providing for disposition to be made of assets in the event of dissolution.

8b) *SSRC* (W. H. Nicholls). No report. G. H. Hildebrand was selected to succeed R. A. Gordon for the term 1961-63.

8c) NBER (W. L. Thorp). No report. It was VOTED to nominate W. L. Thorp for a full five-year term (1965).

8d) AAAS (W. S. Vickrey). A report was read and approved. This report will be published in the May, 1961, *Papers and Proceedings*. It was VOTED to express appreciation to Professor Vickrey for the effective manner in which he has represented our Association to the AAAS meetings. K. E. Boulding was selected for our representative for a three-year term (1962).

9. *Unfinished Business.*

It was VOTED that, if satisfactory arrangements can be assured, the selection of Hotel America in the new Prudential Building be approved for our headquarters in Boston in 1963. No action was taken in the selection of the time and place of meetings for 1965 and subsequent years.

The discussion of the 1960 program, led by P. A. Samuelson, preoccupied the remaining part of the session, which was adjourned for lunch but continued informally thereafter. The problems of size and cost of the *Papers and Proceedings* were discussed, and the proposal was made to limit the number of sessions and in particular the number of sessions for which publication responsibility is to be assumed.

It was VOTED to participate in a joint session with the American Political Science Association at their September meetings, but it was understood that we should assume no publication obligations. A symposium on economic and political science problems of Latin America was proposed as a joint session for the December meetings.

2. Minutes of the Christmas meetings held in St. Louis, Missouri, December 27 and 30, 1960:

The *third meeting* of the *1960 Executive Committee* was called to order at 6:30 p.m., December 27, T. W. Schultz presiding. Others present were: G. L. Bach, J. W. Bell, L. V. Chandler, J. K. Galbraith, R. A. Gordon, B. F. Haley, P. A. Samuelson, James Tobin, and R. A. Young. Absent were: A. F. Burns, A. L. Harris, G. A. Stocking, and Mabel F. Timlin. Present as guests were: Gardner Ackley, Roy Blough, M. A. Copeland, Solomon Fabricant, R. A. Lester, B. W. Lewis, E. S. Mason, J. P. Miller, F. C. Pierson, L. G. Reynolds, and W. L. Thorp. The meeting adjourned at 12:00 p.m.

The *first meeting* of the *1961 Executive Committee* was held on December 30 at 6:00 p.m., P. A. Samuelson presiding. Others present were: G. L. Bach, J. W. Bell, Ewan Clague, Solomon Fabricant, B. F. Haley, R. A. Lester, E. S. Mason, T. W. Schultz, and R. A. Young. Absent were: A. F. Burns, A. L. Harris, and C. P. Kindleberger. Present as guests were: Gardner Ackley, K. E. Boulding, L. V. Chandler, R. A. Gordon, and Theodore Morgan.

1. *President's Remarks* (T. W. Schultz and P. A. Samuelson). T. W. Schultz presented a summary report of his activities during the year: the delegation of program responsibilities to P.A. Samuelson; special developments involving policy matters, such as co-sponsorship in the National Task Force activities, the Continental Classroom TV Program, and the economic internship proposal; and the activities of committees working on economic abstracts, social science textbooks, and other projects. The order of business on the agenda was not followed chronologically. Items calling for action before year-end were taken up first.

2. *Minutes.* The Minutes of the March 25-26, 1960, meeting were approved as presented in mimeographed form.

3. *Report of the Secretary* (J. W. Bell). Local arrangements for the annual meeting have again been put into the hands of a competent staff, Homer Jones, Chairman. We are greatly indebted to facilities provided by the Federal Reserve Bank in St. Louis. In 1961, our relationships with the Federal Reserve Bank in New York will be repeated with the appointment of J. Robert Lindsay as Chairman of the Local Arrangements Committee. We are grateful to the Federal Reserve authorities for permitting us to draw upon their facilities and staff to insure the success of the AEA meetings.

Growth and composition of membership are shown in Exhibit II. The figures indicate the continuation of membership growth, which became accelerated in 1956, when our last *Handbook* was issued. Gratifying as this is, it has complicated the problem of keeping lists up to date and has increased billing costs. Last year's innovation, despite some confusion, resulted in prompter payments though at greater cost. Some addition to our foreign membership can be credited to co-operative arrangements with the Asia Foundation and with the ICA.

A new contract with George Banta Company, Inc., has been agreed upon after obtaining estimates from a number of printing establishments. This contract will not prevent sepa-

rate arrangements for the publication of a directory or handbook and steps are being taken to investigate the feasibility of publishing the next volume of the directory in a form which will permit less costly revisions. In the meantime, as an economy measure, no supplements to the 1956 *Handbook* have been issued since the 1957 supplement.

The information booklet continues to be a useful medium. A less expensive leaflet is contemplated to answer the increasing number of inquiries concerning career outlook and employment opportunities. A report from Richard D. Irwin, Inc., on the "Readings," "Translations," and "Survey" series was submitted but not discussed, except to display a check representing our share of 1960 profits.

The Secretary has been notified that the matter of the registration of the AEA as a charitable fund-raising organization with the Department of Social Welfare of New York State has been dropped.

4. *Reports of the Treasurer, Finance Committee, and Auditor* (J. W. Bell). Copies of the financial statement, income and expense account, investment holdings, and the Auditor's Report were distributed or made available for inspection. The financial results of the year were summarized. Although some expenses have increased, economies were effected, and income—some from unexpected sources—increased even more; so that instead of a balance, the budget showed a substantial net income, with a correspondingly large increase in surplus. The chief sources of income are fully explained in the Treasurer's Report, but it was again pointed out that fortuitous income from profits on sale of securities and net income from annual meetings are not sources which should be relied upon to balance the budget.

The Treasurer submitted the report of the Finance Committee, which contains an analysis of our investment holdings and shows purchases and sales made during the year. Although we have taken substantial profits from sale of securities, our present holdings show material appreciation in market value over last year—an appreciation which contributed substantially to the growth of unappropriated surplus.

Attention was called to an exhibit in the Auditor's Report presenting a statement of foundation funds. Though these funds are indicated as a liability on our statement of financial condition, they are not separated on the asset side but are intermingled, chiefly with investments.

The above reports were accepted, and it was VOTED to reappoint C. Wells Farnham and Corliss D. Anderson as members of the Finance Committee, and the Secretary was instructed to write a letter of appreciation and thanks to these members for their valued services. A similar VOTE of acceptance was passed with respect to the Auditor's Report, and the Secretary was instructed to write a letter of appreciation to David Himmelblau & Company.

5. *Report of the Managing Editor* (B. F. Haley). The report submitted for publication was summarized. It contains material on manuscripts submitted, summary of contents, including information concerning the survey articles made possible by the Rockefeller Foundation grant, subject-matter distribution, actual and budget expenditures, recommended budget for 1961, and nominations of new members of the Board of Editors. It was VOTED to accept the report and approve nominations and the 1961 budget.

Office management problems of both the Editor's and the Secretary's offices were further discussed, and it was VOTED to increase the salaries of Miss Doris Merriam and Miss Gertrude Tait from $6,300 to $6,600.

Successorship, B. F. Haley and J. W. Bell. At this juncture, Messrs. Bell and Haley left the meeting while certain questions pertaining to the positions of Secretary-Treasurer and Editor were discussed. (See 6*i*.)

Mr. Samuelson presented for Arthur F. Burns the report of the Committee on Secretary-Treasurer Successorship, which consisted of himself, T. W. Schultz, G. W. Stocking, and A. F. Burns, Chairman. The Committee made the following recommendations:

a) That J. W. Bell be appointed Secretary Emeritus on his retirement from the Secretary-Treasurership in 1961 and that he be asked to serve as consultant to the Association.

b) That Harold F. Williamson, of Northwestern University, be appointed Secretary-Treasurer in succession to J. W. Bell and that his salary be $8,000 per year.

c) That J. W. Bell be asked to continue to serve as a member of the AEA Finance Committee and that at some later time the Executive Committee consider an appropriate way of recognizing his long and devoted service to the Association.

It was VOTED unanimously to approve the report of the Committee.

Mr. Schultz reported that Bernard F. Haley had declined to accept reappointment as Editor for a regular three-year term, 1962-64. He is willing, however, to serve for one year beyond his present term, i.e., until the end of 1962, in order to permit adequate time for choice of a successor.

It was VOTED to approve Mr. Haley's appointment as Editor for one year beyond his present term.

Mr. Schultz recommended that Mr. Bell's salary as Secretary-Treasurer and Mr. Haley's salary as Editor be increased to $8,000 per year.

It was VOTED to approve this recommendation.

Mr. Bell and Mr. Haley then rejoined the meeting.

6. *Reports of Standing and Special Committees.*

6a) *Committee on Research and Publications* (Gardner Ackley). Supplementing the report which is published below, materials were submitted on the following subjects: the "Readings Series" volume on *Economic Development* is still under way and a volume on *Economic Welfare* is being discussed; a list of foreign-language books which are being considered for translation; and the index of economic periodicals, which is being reported on by J. P. Miller. It was VOTED to accept the Committee's report.

6b) *Cumulative Index Project* (J. P. Miller). Volume I of the five-volume work was exhibited at the meeting by Richard D. Irwin, Inc. The privileged price of $10.50 to members for the set was made possible by AEA appropriations and an additional grant from the Ford Foundation. The subsequent four volumes should be completed by mid-1961. Complimentary copies of Volume I were tendered T. W. Schultz, P. A. Samuelson, and J. W. Bell.

6c) *Committee on Surveys of Foreign Economic Research* (G. H. Hildebrand). No report. (See March, 1960, minutes.)

6d) *Committee on Economic Education* (B. W. Lewis). The study of social science textbooks used in the schools is now nearing completion, under the direction of Paul Olson. The larger study of the social sciences in the schools has been taken over by the ACLS and other councils and some of the Committee's activities have been absorbed by the National Task Force. It was VOTED to accept the Committee's report.

6e) *Committee on Economic Abstracts* (R. A. Gordon). A report was submitted, containing a summary of activities and recommendations. After extended discussion, it was VOTED to authorize the Committee to explore further the willingness of foreign-journal editors to co-operate and perhaps to appoint a capable individual to prepare dummy material which should test the feasibility of the project. The Committee is scheduled to report back at the December, 1961, meeting.

6f) *National Task Force on Economic Education and Continental Classroom* (G. L. Bach). A comprehensive progress report of the meetings held and investigations made by members of the National Task Force was presented and discussed.

The Task Force recommended that the AEA agree to co-sponsor the Continental Classroom TV Program, either in 1961-62 or 1962-63. It was VOTED (by a show of hands, five to one, others not voting) that the AEA co-sponsor the Continental Classroom under the following conditions: (1) that the Executive Committee have a veto over the personnel conducting the program; (2) that the program be postponed to 1962-63; (3) that detailed supervision by the AEA be effected through its members on the National Task Force.

6g) *Committee on the Papers and Proceedings* (Roy Blough). The Committee has continued to make inquiries which would answer the following questions: (1) what should the program look like in 1965 and would the *Papers and Proceedings* volume be appropriate then; (2) to what extent would the *Papers and Proceedings* volume interfere with the kind of program we want in 1965. This suggests that the program is limited by the number of sessions we are able to publish. Other arrangements might be: publish a minimum number of papers and abstract others; abstract all papers or "publish by lot." The Committee expected to report at the spring meeting of the Executive Committee on matters concerning the expense of publication and the quality of papers.

6h) *Committee on Association Deficit* (S. E. Leland). No report. It was VOTED that the Committee be discharged.

6i) *Committee on the Secretary-Treasurer Successorship* (A. F. Burns). See 5.

6j) *Committee on Carnegie Travel Grants* (L. G. Reynolds). It was reported that these grants have served a very useful purpose, and it was recommended that we seek a renewal of the grant. It was VOTED to accept the report and the officers of the Association were authorized to ask the Carnegie Corporation for another grant.

6k) *Committee on Academic Freedom and Civil Liberties* (F. M. Boddy). No report.

6l) *Committee on Professional Ethics* (R. D. Calkins). No report.

6m) *Committee on Honors and Awards* (Clair Wilcox). No report.

6n) *Asia Foundation Grant* (J. W. Bell). The report to the Asia Foundation was briefly summarized. A renewal of the $2,500 grant has subsequently been promised.

6o) *Nominating Committee.* A. F. Burns was appointed Chairman and a list of names was suggested as a panel from which other members might be selected.

7. *Reports of AEA Representatives.*

7a) *IEA.* It was VOTED to extend the terms of Howard S. Ellis and Gottfried Haberler nine months (to September, 1962), at which time their membership on the IEA Council will expire.

7b) *ACLS* (F. H. Knight).

7c) *SSRC* (W. H. Nicholls).

7d) *NBER* (W. L. Thorp).

7e) *AAAS* (K. E. Boulding).

Reports were received from W. H. Nicholls and K. E. Boulding. These and those from F. H. Knight and W. L. Thorp, received after the meeting, are published below.

7f) *Census Advisory Committee.* Solomon Fabricant reported that the Committee has met twice with the Bureau of the Census, once in April and again in November, and that four new members were co-opted for the latter meeting. The names of the Committee members, with two additions, are appended in the Secretary's Report. The next meeting of this Committee is scheduled for spring, 1961. The work of the Council of Population and Housing Census Users, upon which Raymond Goldsmith has served, has now been dissolved.

7g) *IIE.* Theodore Morgan distributed and described summary information on the Economics Institutes. The first was held at the University of Wisconsin in 1958 and the second and third in 1959 and 1960 at the University of Colorado. The fourth session is scheduled at the University of Colorado in July and August, 1961.

8. *Other Unfinished Business.*

8a) The discussion of future meeting places for 1965, 1966, and 1967 was postponed until the spring meeting of the Executive Committee. It was agreed to hold this meeting in New York City, March 24-25.

8b) No action was taken regarding foreign honorary members.

8c) Names for counsel were discussed but no action was taken.

8d) An extension of our agreement with the ICA with respect to foreign members was reported.

9. *New Business.*

9a) It was VOTED to approve a program of economic internships under the administration of Brookings Institution. It is understood that the program will start with three interns and will continue for three years. Further details are included in the brief report by F. C. Pierson published below.

9b) R. A. Gordon spoke briefly about a proposal to announce in the *American Economic Review* openings for foreign visiting economists coming to the United States and who might be seeking teaching positions or lectureships in American colleges or universities.

9c) By consensus of the members present, it was agreed that the title "First Vice-President" be changed to "President-elect" and our bylaws be changed accordingly. It was VOTED to submit this recommendation at the December 30, 1960, business meeting.

The balance of the meeting was devoted to President-elect E. S. Mason's suggestions for the 1961 program.

The meeting adjourned at 9:30 p.m., to be reconvened in New York City on March 24-25, 1961.

ACTIVITIES AND OPERATIONS

Annual Meetings. The final report on the 1959 Washington, D.C., meetings showed a registration of 5,674, of which 2,802, or 34 per cent, were members of the American Economic Association. Many registrants were members of two or more of the participating associations. The allocation of $9,848 was made on the basis of gross registrations, of which the AEA received 42 per cent.

Local arrangements for the annual meeting at the Chase-Park Plaza Hotels were very satisfactory and all operations were effectively conducted by a competent staff, Homer Jones, of the Federal Reserve Bank of St. Louis, Chairman, ably assisted by W. E. Walker, Vice-Chairman. The work of registration, room assignments, printing of the joint program, handling exhibits,

the conduct of the employment register, and making arrangements for special events, all done on a noncommercial basis, would cost an appreciable sum if done by professionals in the convention business, and we are fortunate to be able to draw upon personnel who consider service to the Association a privilege of membership and a contribution to good public relations. In 1961, with J. Robert Lindsay Chairman of the Local Arrangements Committee, we will again call upon the Federal Reserve Bank in New York for their loyal co-operation.

The schedule for future meetings is: 1961, New York City, Hotel Commodore; 1962, Pittsburgh, Penn-Sheraton Hotel; 1963, Boston, Hotel America; and 1964, Chicago, Conrad Hilton Hotel.

Membership. Exhibit II presents an analysis of membership (1960). It shows an increase of 678 members and 194 subscribers, a total of 872. Prior to 1956, the annual increment was about 200, since when the figures show 1,067, 507, 756, and 1,170 for 1956-59, respectively. The bulge in 1959 resulted from a minor membership drive, i.e., processing selected lists of names, and the past year's increase is in part a result of this effort.

Last year's campaign to "help save billing costs" resulted in prompter payments but at greater cost and not without some confusion and complaints. One thousand out of 9,000 members were dropped in April after receiving the March number. These were billed for $1.50, and, through an oversight in the Secretary's Office, many received this bill without the accompanying $6.00 bill for reinstatement in full standing; hence some paid $1.50 for the March billings, were then billed for $4.50 for reinstatement; others paids the $1.50 plus $6.00, which necessitated a refund. The final drops numbered about 400. An expiration notice was sent with bills for 1961 dues—bills which we attempted to send out at the end of the fiscal year. Lack of help protracted billings into the holiday season.

Co-operative arrangements with the ICA and the Asia Foundation have enabled us to expand foreign memberships and subscriptions. On recommendation of the ICA, some 90 foreign economists have been added to our membership roll. This agreement has been renewed for 1961. The Asia Foundation grant provides a subsidy to aid Asian students in American universities and colleges to attend and participate in our annual meetings and to become members on a complimentary or half-rate basis for one year. Thirteen availed themselves of travel subsidies and about a hundred of membership privileges. Through the courtesy of Freedom House, some 200 ten-volume sets of the *American Classics* have been allocated to foreign scholars. The Secretary's Office helped supply names in this worthy enterprise.

Publications. In the course of years, the size of the inventory of our publications has increased to impressive proportions. Some seven and a half tons of back numbers are stored in warehouses by George Banta Company, Inc. Back numbers out of print are also increasing, as indicated by items starred in our publication list. This fact has attracted the attention of reprint companies, who are anxious to reproduce out-of-print numbers. So far, we have been able to fill these gaps by advertising in the *AER*. It may become desirable in the

course of time to allocate the handling of all back numbers to some specialized corporation. Some societies have done this. However, we continue to get excellent service from the George Banta Company, Inc., and take this into account when renewing contracts involving increased printing costs. A new contract, involving a 7 to 10 per cent increase, goes into effect in 1961.

American Economic Review. The Managing Editor's report, covering the size, contents, and cost of the *Review,* budget estimates, changes in the Editorial Board, and so forth, is published below. The concern which the Secretary's Office has with the *AER* relates chiefly to permission to reprint and translate, the advertising section, the section devoted to the announcement of vacancies and applications, and over-all use of the mailing list.

Our policy in accepting advertising is to make this section as useful as possible to our members and still produce income. We have made little effort to solicit institutional ads for revenue. The section is devoted to exchange advertisements with other journals (we insist on up-to-date contents being furnished) totaling about 70 pages in 1960, and the announcement of new publications which should be of interest to members. This paid section numbered about 130 pages in 1960. We have increased the lineage somewhat, but the much larger income received from advertising results principally from the new rates established in our 1960 rate card. This schedule increased rates some 16 per cent—about equal to our increase in costs, with no allowance for larger circulation. We do not consider it advisable to charge publishers "all the traffic will bear."

The announcement section of vacancies and applications continues to be a serviceable contribution to those seeking employment or a change of jobs and to employers of economists—academic, governmental, and business. No effort is made to follow up inquiries; so we have no measure of its effectiveness except occasional letters of appreciation from those who inform us not to repeat their announcement because of satisfactory results obtained. This service supplements the employment register conducted at the annual meetings in cooperation with the USES. We have virtually put the management of the employment register in the hands of the USES because of the increasing size and cost of this operation. The employment register is a complicated and difficult service to conduct and under the circumstances the job is extremely well done. However, this is not the best solution to the placement problem, since the USES operates on a state rather than a national basis and more on a vocational than on a professional basis. Moreover, professional economists are accustomed to deal through university departments and personnel and not through a government agency. Both announcements and employment register must be continued but they are inadequate substitutes for a full-fledged year-round placement service. We are not prepared to undertake such service. However, the volume of inquiries and requests which we receive is ample evidence of the real need for some central clearing house for the supply of and the demand for economists. The demand and supply aspects of the market for economists was explored at a round table session at the 1958 meetings. It provoked lively interest but did not result in the establishment of a centralized bureau under the auspices of all the social science associations or councils or

other private organizations or government agencies. This proposal needs to be revived.

Papers and Proceedings. The size and cost of the May, 1961, volume will again exceed expectations and budget estimates. The Committee on the *Papers and Proceedings* is giving attention to this problem and will present at the March, 1961, meeting of the Executive Committee a report on how to improve quality and reduce costs.

Handbook or Directory. No supplement to the 1956 *Handbook* has been issued since the 1957. These supplements are expensive to issue and it has been reported that they are not very useful. The insistent demand is for a revision of the "who's who" type of directory, since the issue soon becomes dated. The current, up-to-date mailing list of names and addresses which can be furnished does not serve a general purpose. It has been proposed that the directory be published more frequently and, if necessary, at an extra charge to members. It might be interesting to learn how many would buy the volume at cost, but such a policy lacks the advantages of general distribution which makes the volume universally available. Whatever our policy, preparation for a revision of a "who's who" directory must be made a year in advance unless some more economical revolving system is introduced. We are exploring the feasibility of holding type for periodic revision or adopting a more flexible lithograph method of publication.

Permission to Reprint and Translate. We continue to grant quite freely permission to quote, paraphrase, reprint, or translate material published in the *Review* and the *Papers and Proceedings.* A record is kept of requests by author or other parties, for use in articles, books, readings, or other publications, for educational, classroom use or for commercial publications. If the use is for profit-making purposes, a conventional stipend or token payment is suggested. These payments are passed on to authors.

Use of Mailing List and Direct Advertising. In 1957, the Secretary's Office took over from Banta the function of addressing our publications and communications. We purchased an Elliott Addressing Machine, save several thousand dollars a year expense in sending out mailings at least six times a year, and earn a substantial income by selling the list to direct mail advertisers. These are chiefly publishers, but research organizations, government departments, business corporations, and labor organizations are also customers. Unfortunately it has not proved feasible to classify our list of members on Elliott stencils by fields of specialization. Our list is classified only geographically. It has not proved possible, therefore, to limit the mailing of items which might be of special interest to a specific group or groups. We have had to act on the policy of approving requests which have sufficient merit and value to a large enough segment to justify mailing to all members. Some items have provoked irate criticism. We are glad to have members express their reactions, but we ask their indulgence. What may offend one may please another and the benefit to the whole is our objective. A list of permissions granted is appended to this report.

We keep a complete record of all advertising, namely, purpose, cost, and income from advertising, both in our publications and by direct mail.

Information Booklet. The 1960 revision of this useful twenty-four page leaflet was used partly in membership promotion but principally in correspondence relating to professional guidance. We found it necessary to order 1,000 copies plus 500 extras to satisfy this growing need. Along with the information booklet, we have made use of reprints of Exhibit II from the 1948 *Directory* (reprinted in the 1953 *Handbook*) and Exhibit IV of the 1956 *Handbook*. These exhibits present a description and sources of information concerning the profession of economist. They are expensive media and we have need of a more economical form of leaflet on career outlook for economists to satisfy an apparently growing demand from students seeking guidance information.

Committee Activities. The list of members of all standing and special committees is found at the end of this report. The reader is referred to the minutes of the Executive Committee meetings and to reports of the committee chairmen for a more complete record than is outlined here.

Research and Publications (Gardner Ackley). Several meetings of this reconstituted committee have been held. Some of the projects, such as the index of economic periodicals, are being brought to a conclusion; others, like the "Readings" and "Translations" series, are being continued; and suggestions of new projects are under consideration. The first volume of the cumulative index, prepared under the direction of J. P. Miller, was on exhibition at the annual meetings and the publisher, Richard D. Irwin, Inc., is accepting orders for the five-volume set at a privileged price of $10.50 to members. This bargain price is made possible by foundation grants as well as AEA appropriations.

By special arrangement with Richard D. Irwin, Inc., publishers of the "Readings Series," a renewal of the special sale of selected books sponsored by the AEA was announced at the Christmas meetings. The purpose of this special offer is to enable old members to fill gaps in their collection of AEA-sponsored volumes and new members to obtain a complete package of the thirteen volumes published—these at greatly reduced prices from the regular list price.

Surveys of Foreign Economic Research (G. H. Hildebrand). A report was submitted at the March Executive Committee meeting, describing arrangements made for the first six essays contracted with authors, chiefly foreign nationals writing about current economic research in their own country.

Economic Education (B. W. Lewis). The textbook project, under the direction of Paul R. Olson, is nearly completed. A co-operative study of the social sciences in secondary schools, joint with the ACLS, the Joint Council on Economic Education, and the National Science Foundation, is still in its preliminary stages. The register of economists in economic education, compiled from the 1956 *Handbook* questionnaires, is still available for use. Other activities of this Committee have been taken over by newly established committees.

National Task Force on Economic Education; Continental Classroom TV Program. When the Committee on Economic Education was first established, its primary interest was that of economic education on the secondary school level and what could be done to better prepare students for the study of eco-

nomics on the college level. The subject had such broad application that it was referred to the SSRC and the Committee turned its attention to economic education on the collegiate level and produced the report, *The Teaching of Undergraduate Economics*, Horace Taylor, Chairman, December, 1950. This accomplished, the Committee turned its attention to economic education on the postgraduate level. Its report, *Graduate Training in Economics*, by Howard R. Bowen, was published in September, 1953. Thereafter the Committee again turned its attention to economic education on the secondary school level. The interest in this subject has become so widespread that the time has come for co-operative effort, and, at the suggestion of the Committee for Economic Development, a so-called "National Task Force" was appointed by T. W. Schultz, President of the AEA, and sponsored by CED, which consists of five AEA members and two consultant members from the field of education, with an Executive Secretary from the CED. This group will work independently of both the AEA and the CED but it will submit reports for publication in the "Proceedings."

On recommendation of the Task Force, the Executive Committee acted at its December meeting in favor of AEA co-sponsorship of the so-called "Continental Classroom TV Program" under conditions which involve detailed supervision through its members on the National Task Force and veto control over personnel conducting the program. This innovation will be watched with interest. (See G. L. Bach's paper in the session on "Economic Education: Challenge to our Profession" for a fuller account of these projects.)

Economic Abstracts (R. A. Gordon). A committee has been appointed to investigate the feasibility of publishing periodic abstracts of articles in selected economic journals, domestic and foreign, and a report will be expected at the December, 1961, meetings. This committee is also reviewing the findings of previous committees on data depositories (chiefly of government materials) and is considering what, if anything, should be done to clear information on lecture and teaching opportunities to aid foreign scholars coming to the United States in establishing suitable contacts.

Association Deficit (S. E. Leland). The duties of this Committee have been performed. On recommendation of its Chairman, the Committee was discharged.

Papers and Proceedings (Roy Blough). The Committee is scheduled to report at the March meeting, 1961, on ways and means of limiting the size and expense of publishing the volume and improving the quality of its contents.

Successorship, B. F. Haley and J. W. Bell. See Executive Committee minutes for action taken.

Carnegie Travel Grants (L. G. Reynolds). Satisfactory experience with this grant prompted action authorizing officers to ask for new funds.

Academic Freedom and Civil Liberties (F. M. Boddy). No new cases have arisen this year and the Committee submitted no report.

Professional Ethics (R. D. Calkins). No report, but a report will be submitted at the March meeting.

Honors and Awards (Clair Wilcox). No report. A report scheduled for March.

Foreign Honorary Members. No further action taken by the Nominating Committee on this matter. A committee on foreign honorary members has not been reconstituted.

New Business and Miscellaneous Reports.

Counsel for AEA. Although a number of names were suggested and discussed, no selection has been made.

Economic Internships. A program of economic internships under the administration of the Brookings Institution has been approved by the AEA. The program will parallel one which has been in operation for some years and which has been administered by the American Political Science Association but will start on a much more modest scale. The program is described in F. C. Pierson's report.

IEA. Terms of representatives Howard S. Ellis and Gottfried Haberler were extended. A report submitted too late to be presented at the meeting is published below.

IIE (Theodore Morgan). See report.

Census Advisory (Solomon Fabricant). The Committee has met with the Bureau of the Census staff and has added new members to make its work more effective.

Reports of our representatives to the SSRC, ACLS, NBER, and AAAS are published below.

Standing Committees

COMMITTEE ON ACADEMIC FREEDOM AND CIVIL LIBERTIES

Francis M. Boddy, *Chairman* (1960)
Howard R. Bowen (1961)
Walter D. Fisher (1962)

COMMITTEE ON ASSOCIATION DEFICIT

Simeon E. Leland, *Chairman*
Howard R. Bowen
Milton Friedman
James Washington Bell ⎫ Ex Officio
Bernard F. Haley ⎰

COMMITTEE ON ECONOMIC EDUCATION

Ben W. Lewis, *Chairman* (1962)
Paul J. Strayer (1960)
Emanuel T. Weiler (1960)
Clark C. Bloom (1961)
Floyd A. Bond (1961)
Laurence Leamer (1961)
Clark L. Allen (1962)
John R. Coleman (1962)
Kenyon Knopf (1962)

COMMITTEE ON HONORS AND AWARDS

Clair Wilcox, *Chairman* (1962)
Paul A. Samuelson (1962)
Gardner Ackley (1964)
Fritz Machlup (1964)
Martin Bronfenbrenner (1966)
Earl J. Hamilton (1966)

COMMITTEE ON THE *Papers and Proceedings*

Roy Blough, *Chairman*
J. Douglas Brown
Joseph W. Conrad
William J. Fellner
Milton Friedman

COMMITTEE ON PROFESSIONAL ETHICS

Robert D. Calkins, *Chairman* (1961)
Richard Ruggles (1961)
Joseph Dorfman (1961)

COMMITTEE ON RESEARCH AND PUB-
LICATIONS

Gardner Ackley, *Chairman* (1962)
R. A. Gordon (1960)
William H. Nicholls (1962)
Willard L. Thorp (1965)
James Washington Bell, Ex Officio

COMMITTEE ON SECRETARY-TREAS-
URER SUCCESSORSHIP

Arthur F. Burns, *Chairman*
Morris A. Copeland
Paul A. Samuelson
Theodore W. Schultz
George W. Stocking

COMMITTEE ON SURVEYS OF FOREIGN
ECONOMIC RESEARCH

George H. Hildebrand, *Chairman*
Abram Bergson
Martin Bronfenbrenner
Emile Despres
Robert J. Lampman

COMMITTEE ON CARNEGIE TRAVEL
GRANTS

Lloyd G. Reynolds, *Chairman*
(1960)
Frederick H. Harbison (1960)
James Washington Bell ⎱ Ex Officio
Theodore W. Schultz ⎰

CUMULATIVE INDEX ADVISORY COM-
MITTEE

John Perry Miller, *Chairman*
Robert Bishop
Earl J. Hamilton
Fritz Machlup
Joseph J. Spengler

INSTITUTE OF INTERNATIONAL EDU-
CATION, ADVISORY AND POLICY BOARD

Theodore Morgan, *Chairman*
Rendigs T. Fels
Carter Goodrich
Michael Hoffman
D. Gale Johnson
Irving B. Kravis
Lorie Tarshis

Council and Other Representatives

AAAS

Kenneth E. Boulding (1962)

ACLS

Frank H. Knight (1962)

IEA REPRESENTATIVES

Howard S. Ellis (1961)
Gottfried Haberler (1961)

NBER

Willard L. Thorp (1965)

SSRC

R. A. Gordon (1960)
Gardner Ackley (1961)
William H. Nicholls (1962)

Committees Appointed During the Year

CENSUS ADVISORY COMMITTEE

Solomon Fabricant, *Chairman*
(1963)
Harold Barger (1961)
John Lintner (1961)
Millard Hastay (1962)
Anthony M. Tang (1962)
H. Gregg Lewis (1963)

COMMITTEE ON ECONOMIC ABSTRACTS

Robert A. Gordon, *Chairman*
Gardner Ackley
William H. Nicholls
Joseph J. Spengler

COMMITTEE ON ELECTIONS

Herbert V. Prochnow, *Chairman*
Arthur R. Tebbutt
James Washington Bell, Ex Officio

NATIONAL TASK FORCE ON ECONOMIC
EDUCATION
ECONOMIST MEMBERS
  George Leland Bach, *Chairman*
  Floyd A. Bond, *Executive Secretary*
  Lester V. Chandler
  Robert A. Gordon
  Ben W. Lewis
  Paul A. Samuelson

CONSULTING MEMBERS
  Arno Bellack
  M. L. Frankel

FINANCE COMMITTEE
  C. Wells Farnham, *Chairman*
  Corliss D. Anderson
  James Washington Bell

NOMINATING COMMITTEE
  George W. Stocking, *Chairman*
  Vincent W. Bladen
  Everett E. Hagen
  C. J. Hitch
  Frank C. Pierson
  Edwin Young

Representatives of the Association on Various Occasions

AMERICAN ACADEMY OF POLITICAL AND SOCIAL SCIENCE
  April Meeting
    Howard M. Teaf, Jr.
    Joseph H. Willits
AMERICAN POLITICAL SCIENCE ASSOCIATION MEETING
    A. C. Harberger
BOWLING GREEN STATE UNIVERSITY, GOLDEN ANNIVERSARY
    Jacob Cohen
UNIVERSITY OF CONNECTICUT, SILVER CONVOCATION HONORING ALBERT N. JORGENSEN
    W. Harrison Carter
DUNBARTON COLLEGE OF HOLY CROSS, TWENTY-FIFTH ANNIVERSARY CONVOCATION
    Ralph A. Young
HARPUR COLLEGE, DEDICATION OF NEW CAMPUS
    Melvin Seiden
INTERNATIONAL INSTITUTE OF EDUCATION FOURTH NATIONAL CONFERENCE ON EXCHANGE OF PERSONS
    Howard S. Ellis
    Robert A. Gordon
NATIONAL EDUCATION ASSOCIATION
  San Diego Conference
    Clark C. Bloom
    James Calderwood
INAUGURATION OF UNIVERSITY AND COLLEGE PRESIDENTS
  Reamer Kline, Bard College
    George Dalton
  Theodore August Rath, Bloomfield College
    Ralph N. Calkins
  Arend Donselaar Lubbers, Central College
    Donald T. Butler

Judson William Foust, Central Michigan University
  Michael J. L. O'Connor
Garland A. Godfrey, Central State College
  Anwar Nissan
Robert Edward Lee Strider, II, Colby College
  Walter N. Breckenridge
Joseph Franklin Marsh, Jr., Concord College
  W. W. Creason
Leo Warren Jenkins, East Carolina College
  James H. Stewart
Donald Ezzell Walker, Idaho State College
  Robert C. Stevenson
William Graham Cole, Lake Forest College
  James Washington Bell
Owen Meredith Wilson, University of Minnesota
  O. H. Brownlee
Arlo Leonard Schilling, North Central College
  J. H. Noble
Robert Kenneth Carr, Oberlin College
  Ben W. Lewis
Paul Hampton Morrill, Park College
  Jerzy Hauptmann
Clarence Russel Moll, Pennsylvania Military College
  W. J. Zahka
John Addison Howard, Rockford College
  Eveline M. Burns
Andrew David Holt, University of Tennessee
  F. B. Ward
James Earl Rudder, Texas A. and M. College
  Roland J. Hildreth
Mahlon Albert Miller, Union College
  James W. Martin

Use of the Mailing List

Permission was granted to the following to use our mailing list to send the material indicated:

SAMUEL AMBARAS: Catalogues.

AMERICAN BANKERS ASSOCIATION: *Economic Growth without Inflation* and *Economic Growth, Inflation and You*

AFL-CIO: *Regarding Study of Employment, Growth and Price Levels;* announcement of availability of publications and trade-union speakers

AMERICAN FINANCE ASSOCIATION: "Essential Financial Policies for Sustainable Economic Growth," by Robert B. Anderson, from *Commercial and Financial Chronicle*

AMERICAN PHILOSOPHICAL SOCIETY: Announcement of *Population Redistribution and Economic Growth, U.S., 1870-1950*

ATLANTIC MONTHLY: Subscription offer

BANKERS TRUST COMPANY: *Credit Expansion and Interest Rate,* by Roy L. Reierson

BRITISH INTERNATIONAL ADDRESSING: Announcement of literature published in Great Britain on international economic trends

BROOKINGS INSTITUTION: Announcement of spring publications

UNIVERSITY OF CALIFORNIA PRESS: Announcement of *An Introduction to the Theory of Interest*

CAMBRIDGE UNIVERSITY PRESS: Announcement of *Stage of Economic Growth, a Non-Communist Manifesto*

CARNEGIE CORPORATION OF NEW YORK: April and October *Quarterly*

CHESAPEAKE & OHIO RAILROAD: *1959 Annual Report*

UNIVERSITY OF CHICAGO PRESS: Announcements of *Social Change in the Industrial Revolution,* of *Economic Development and Cultural Change,* and of series on economic theory, "Oxford-Cambridge Handbooks"; subscription offer, *Journal of Political Economy*

CHRISTIAN CHILDREN'S FUND: Solicitation of funds

COMMITTEE FOR ECONOMIC DEVELOPMENT: Offer of *Essays in Federal Taxation* and *Prices, Costs and Output—1947-57; Guiding Metropolitan Growth, Balance of Payments,* and *National Objectives and the Balance of Payments Problem, CMC Progress Report;* announcement of the creation of a National Task Force on Economic Education; offer of CED publications

COUNCIL ON FOREIGN RELATIONS: Announcement of *The Schuman Plan*

THE ECONOMIST: Subscription offer

ENCYCLOPAEDIA BRITANNICA: Announcement of *Library of Great American Writing*

FORD FOUNDATION: Announcement of program of Faculty Regional Research Seminars in Economics

FORDHAM UNIVERSITY PRESS: Announcement of *Program for Monetary Stability,* by Milton Friedman

FORTUNE MAGAZINE: Subscription offer

THE FREE PRESS; QUADRANGLE BOOKS: Announcement of publications

GENERAL ELECTRIC COMPANY: *Annual Report, An Introduction to General Electric, The Better Life, 1960's Different Problems, Productivity, Complex Problems of Employment Security,* and *Share Owners Quarterly*

HILL AND KNOWLTON: *The Better Life* and reprint of article by Jules Bachmann from *Commercial and Financial Chronicle*

HUMBLE OIL AND REFINING COMPANY: *Percentage Depletion for Petroleum Production* and *Analysis of the Domestic Oil Industry,* by Richard J. Gonzalez

INSTITUTE OF LIFE INSURANCE: *1960 Life Insurance Fact Book*

MACALESTER COLLEGE: *Underwriting Prosperity,* by Harold Lunde

MCGRAW-HILL BOOK COMPANY: Announcement of *The New Inflation,* by W. L. Thorp and R. E. Quandt

UNIVERSITY OF MICHIGAN, BUREAU OF INDUSTRIAL RELATIONS: *Michigan Index to Labor Union Periodicals*

NATIONAL ASSOCIATION OF MANUFACTURERS: *Primrose Path of Centralism*

NATIONAL PLANNING ASSOCIATION: Publicity material

NEW YORK STATE EXECUTIVE CHAMBERS: Reprint of speech by Governor Nelson Rockefeller

PRUDENTIAL INSURANCE COMPANY OF AMERICA: *Prudential's Economic Forecast for 1960, Prudential's Economic Forecast for 1961,* and reprint, "Can We Win the Economic Race with Russia," by Carrol M. Shanks

THE REPORTER MAGAZINE: Subscription offer

ROOSEVELT UNIVERSITY: Announcement of lectures by D. M. Keezer

SATURDAY REVIEW: Subscription offer

UNESCO: Announcement of publications in the field of the social sciences

WAYNE STATE UNIVERSITY PRESS: Announcement of *An Econometric Model of Postwar State Industrial Development,* by W. R. Thompson and J. M. Mattila, and of new teaching assistantships, fellowships, and Ph.D. programs

Respectfully submitted,

JAMES WASHINGTON BELL, *Secretary*

EXHIBIT I
PUBLICATION COSTS

	PAPERS AND PROCEEDINGS			HANDBOOKS		
Year*	Number of Pages	Number of Copies	Cost	Number of Pages	Number of Copies	Cost
1931	308	4,300	$ 1,919	88	4,200	$ 589
1936	360	4,200	2,037	58	4,100	454
1940	444	4,900	2,657	108	5,000	822
1942	548	5,400	3,909	208	5,500	1,775†
1943	535	5,500	3,652			
1944	470	5,800	3,350			
	144	5,900	1,215‡			
1945	536	6,400	4,502			
1946	960	6,700	8,149	143	6,900	2,035
1947	781	7,700	8,140			
1948	591	8,500	8,701	345	7,700	6,948†
1949	537	9,500	7,844			
1950	650	10,100	9,864	41	9,200	1,163†
1951	816	10,400	11,965	18	8,300	692†
1952	768	10,700	13,190	11	8,188	620†
1953	612	10,900	10,935	187	8,400	4,416
1954	765	11,000	13,932	11	7,900	660†
1955	711	11,000	12,900	8	8,000	540†
1956	651	11,200	12,115			
1957	754	12,400	16,253	548	10,100	15,815†
1958	677	12,700	15,471	32	9,300	1,434†
1959	689	14,000	16,780			
1960	745	14,800	18,914			

* This is the year of publication and pertains to the meeting of the preceding year. The figures are published in the subsequent year.
† "Who's who" volumes; 1950 and 1958—"Who's who" supplements; others—names and address supplement.
‡ Part of papers presented at annual meeting published as supplement to June number.

EXHIBIT II
MEMBERS AND SUBSCRIBERS

	Totals 11/30/59	Added	Removed	Gain or Loss	Totals 11/30/60
Class of membership:					
Annual............	8,910	1,141*	651§	490	9,400
Junior............	890	722†	556*	166	1,056
Family............	147	11	8	3	150
Complimentary.....	81	2	8	6	75‡
Life..............	117	30	5	25	142
Honorary..........	14	—	—	—	14
Total members....	10,159	1,906	1,228	678	10,837
Subscribers..........	3,859	918	724	194	4,053
Totals...........	14,018	2,824	1,952	872	14,890

* Includes 156 junior members changed to annual.
† Includes 30 annual members changed to junior.
‡ Includes 19 who do not receive publications.
§ Resigned, 84; nonpayment, 457; died, 18; lack of address, 62; changed to junior members, 30.

REPORT OF THE TREASURER FOR THE YEAR
ENDING NOVEMBER 30, 1960

The financial results are shown in the accompanying tables, with comparable figures for 1959 and 1960. The first table shows comparative financial condition and the second comparative income and expense operations, with budget estimates for 1961.

Financial Operations. Income from all sources for 1960 amounts to $138,943, an increase over 1959 ($118,980) of $19,963, and net expenses of $111,827, an increase over 1959 ($108,345) of $3,482. Net operating income of $27,116 shows an increase of $16,481 over the $10,635 figure for 1959. After accounting for appropriations and allowing for accrued liability for the directory or handbook, the net for 1960 amounts to $22,616, an increase of $16,831 over the figure of $5,785 for 1959.

A comparison of items showing sources of income for 1959 and 1960 indicates that increased income was derived principally from memberships and subscriptions ($5,072), advertising ($6,028), and from the investment account ($8,185). These figures reflect a continued growth of membership, an increase in advertising rates, interest receipts from the investment of temporary funds, and profits on the sale of securities.

In making an appraisal of income and expenses, the reader will find it informing to refer to Exhibit I of the Report of the Committee on Association Deficit, which presents a statement of income and expenses over the eleven-year period, 1948 through 1958 (*Papers and Proceedings,* May, 1959, page 656), and the Treasurer's Report in the May, 1960, *Papers and Proceedings* (pages 682-83). Reference is also made to page 687 of the same citation, where the Treasurer's recommendation for increased dues and subscription rates is reiterated. It hardly seems necessary at this point to reinforce this recommendation by more repetition.

The following figures will bring up to date the table (Exhibit II) found in the Report of the Committee on Association Deficit, showing the relation of investment income to total income from all sources. These are given by amounts and percentages.

| Year | Total Income from All Sources | INVESTMENT INCOME | | | | | | | Per Cent of Investment to Total Income |
		Interest	Per Cent	Dividends	Per Cent	Sale of Securities	Per Cent	Total	
1959	$118,890	$3,895	3.27	$3,231	2.72	$ 7,469	6.45	$14,360	12.15
1960	138,943	6,693	4.81	3,772	2.71	12,392	8.91	22,545	16.43

Most of our expenses increased during 1960, as is shown by items reflecting the cost of printing and expenses of the Editorial Office of the *Review.* Some expenses, however, have been reduced; so the total increase in expenses shows a modest amount of $3,482. It should be noted here that the annual meeting in-

stead of being an expense has again proved to be a source of income (even larger by $1,631 and with a $2.00 registration fee in effect).

Budget Analysis. The financial results of 1960 were better than anticipated, both because we were able to effect some economies, which we may not be able to repeat, and because some unexpected sources of income materialized.

The above statement looks more favorable than it really is, when account is taken of certain contingent commitments which we have made and of which no record appears in this year's Auditor's Report; namely, cumulative index, $6,500 plus $2,500; Walras project, $2,500; IEA, $400. To this total of $11,900 should be added any costs which may result from our sharing the underwriting of 600 copies of the UNESCO volume, *University Teaching of the*

COMPARATIVE FINANCIAL CONDITION, 1951, 1959, 1960

	11/30/51	11/30/59	11/30/60
Assets			
Cash on deposit and on hand..........................	$ 15,058	$ 14,552	$ 16,094
Receivables (net).......................................	2,668	8,552	10,455
Prepaid expenses and inventories........................	658	623	646
Furniture and fixtures (net).............................	1,421	3,031	2,168
Investments at cost:			
Bonds...	43,340	175,617	160,508
Stocks..	49,764	67,651	94,910
Total assets......................................	$112,909	$270,026	$284,781
Liabilities and Funds			
Accounts payable.......................................	$ 6,031	$ 11,893	$ 12,066
Deferred income..	7,383	14,430	12,803
Membership extension fund..............................	855	—	—
Outside grants:			
Fund for Committee on Graduate Training in Economics.	2,204	—	—
Carnegie Fund for International Travel.................	—	6,662	2,539
Ford Fund for index of economic journals..............	—	551*	931
Ford Fund for register of economists..................	—	1,841	1,836
Asia Foundation Fund.................................	—	1,855	1,243
American Economic Review survey articles.............	—	1,775	4,375
Secondary School Social Studies Survey................	—	24,631	12,681
Foreign Economic Research Translations................	—	25,000	25,000
Foreign Economic Surveys.............................	—	40,000	39,085
Fund for Committee on Research and Publications.....	7,172	382	353
Committee on Economic Education......................	—	432	108
Sundry..	—	54	623
Committee on *Papers and Proceedings*..................	—	250	250
Committee appropriations (not expended)................	2,243	—	—
Life memberships.......................................	5,650	12,300	14,100
Total liabilities and funds........................	$ 31,538	$140,954	$127,993
Surplus			
Balance at beginning of period..........................	$ 68,606	$118,987	$129,072
Transfers from life memberships........................	50	300	1,100
Net income or loss for period...........................	12,715	9,785	26,616
Unappropriated surplus.............................	$ 81,371	$129,072	$156,788
Total footings....................................	$112,909	$270,026	$284,781

* Denotes red.

Social Sciences: Economics. We should keep these commitments in mind before making new appropriations based upon larger net operating income.

The 1961 budget figures allow for some increase in expenditures, e.g., printing costs and salaries, and we expect increased income, also, from normal sources. The result should closely balance the budget.

Financial Condition. The first table, on comparative financial condition, sets forth the changes that have occurred in assets and liabilities, 1960 compared to 1959 and for 1951. These changes call for no special comment except for the

COMPARATIVE RESULTS OF OPERATIONS, 1951, 1959, AND 1960

	11/30/51	11/30/59	11/30/60	1961 Budget
Income				
Membership dues.....................	$39,812	$ 55,214	$ 59,208	$ 60,000
Subscriptions........................	15,553	22,272	23,360	24,000
Sales...............................	2,251	3,305	3,456	3,500
Advertising..........................	7,424	14,948	20,976	21,000
Republications income................	1,854	—	723	5,500
Sale of mailing list..................	—	8,524	8,521	10,500
Sundry income.......................	—	357	154	300
Dues and publications income........	$66,894	$104,620	$116,398	$124,800
Interest.............................	$ 1,026	$ 3,895	$ 6,693	$ 4,000
Dividends............................	4,608	3,231	3,772	3,500
Less custodian fees..............	157*	235*	312*	—
Sales of securities (net)..............	2,788	7,469	12,392	—
Investments (less fees)..............	$ 8,265	$ 14,360	$ 22,545	$ 7,500
Total income....................	$75,159	$118,980	$138,943	$132,300
Expenses				
Office salaries.......................	$12,735	$ 28,697	$ 28,046	$ 32,300
Other administrative expenses..........	3,604	10,289	9,010	10,000
Annual meeting......................	501	3,494*	5,125*	—
Executive Committee.................	1,016	1,687	2,173	2,200
Other committee expenses.............	583	881	822	800
Administrative and operating expenses.	$18,439	$ 38,060	$ 34,925	$ 45,300
Review printing.....................	$20,255	$ 37,521	$ 39,429	$ 45,200
Papers and Proceedings printing........	11,965	16,780	18,915	21,000
Directory printing...................	693	—	—	—
Editorial office (*Review*):				
Contributors.......................	1,654	1,788	2,866	2,500
Editorial and clerical salaries.........	7,785	12,787	15,166	17,600
Other expenses (net)................	553	1,409	525	800
Publications......................	$42,905	$ 70,285	$ 76,901	$ 87,100
Total expenses...................	$61,344	$108,345	$111,827	$132,400
Net operating income or loss..........	$13,815	$ 10,635	$ 27,116	$ 100*
Appropriations.....................	1,100*	850*	500*	—
Accrual liability of *Directory*........	—	4,000*	4,000*	4,000*
Net income or deficit..............	$12,715	$ 5,785	$ 22,616	$ 4,100*

* Denotes red.

investments and foundation funds. The Auditor has set up a statement of special funds in Exhibit 3 which itemizes grants received from foundations. These amount to roughly $88,000. On the asset side of the AEA statement, these

INVESTMENT PORTFOLIO

| Year | AT PAR | COST | | | MARKET |
	Bonds	Bonds	Stocks	Total	Stocks and Bonds
1925	$ 25,000	$ 24,661		$ 24,661	
1930	31,000	32,439		32,439	
1933	33,500	32,962	$ 3,954	36,916	$ 31,522
1935	16,000	15,280	28,114	43,394	50,338
1940	25,000	22,519	41,155	63,675	60,553
1942	27,000	24,651	41,556	66,207	58,211
1945	40,000	36,705	44,955	81,661	103,574
1948	35,000	33,108	48,624	81,732	84,841
1950	35,000	33,108	51,978	85,087	104,177
1951	43,000	43,340	49,764	93,104	117,316
1952	42,000	42,312	58,934	101,246	130,836
1953	68,000	68,308	46,458	114,766	134,562
1954	61,000	61,518	38,082	99,600	132,280
1955	75,000	75,370	59,394	134,764	166,772
1956	75,000	75,370	60,237	135,607	168,337
1957	75,000	75,370	55,084	130,454	151,638
1958	75,000	75,370	67,741	143,111	175,609
{1959	75,000	75,386	67,652	143,038	191,506
{1959*	175,000	175,616	67,652	243,268	291,506
1960*	160,000	160,508	94,910	255,418	299,768

* Includes bonds held in temporary operating fund.

RETURN ON INVESTMENTS

Year	Bonds	Stocks	Total	Rate of Return on Cost
1925	$1,350		$ 1,350*	
1930	1,695		1,695	5.22%
1933	1,679	$ 108	1,788	4.84
1935	1,022	680	1,703	3.92
1940	1,037	2,182	3,220	5.06
1942	1,306	2,186	3,492	5.28
1945	1,479	2,488	3,968	4.71
1948	1,194	2,944	4,139	5.06
1950	1,117	3,860	4,977	5.85
1951	1,026	4,607	5,633	6.05
1952	1,117	3,681	4,799	4.75
1953	1,435	3,587	5,022	4.36
1954	1,621	2,961	4,582	4.58
1955	1,750	3,002	4,752	3.53
1956	1,770	3,336	5,106	3.76
1957	1,770	3,397	5,167	3.90
1958	1,770	3,182	4,952	3.46
{1959†	2,518	3,231	5,749	3.90
{1959§	3,894	3,231	7,125	2.90
1960§	6,693	3,772	10,465	4.09

* Estimated income for year.
† Does not include income from bonds held in temporary operating fund.
§ Includes income from bonds held in temporary operating fund.

funds are not identified separately from Association funds, but they are to be found chiefly in the investments and the amounts so employed are identified in the Report of the Finance Committee.

The unappropriated surplus of $129,072 in 1959 has been increased by the amount of the net income and transfer of life memberships to $156,788 in 1960.

The value of our investment holdings of stocks and bonds is shown in the accompanying table, from 1925 to date, and in another table is shown the return on these investments, based on cost. The list of our holdings, together with the changes made during the year, is found in the Report of the Finance Committee.

Respectfully submitted,

JAMES WASHINGTON BELL, *Treasurer*

REPORT OF THE FINANCE COMMITTEE

December 21, 1960

Executive Committee
American Economic Association
Evanston, Illinois

GENTLEMEN:

The accompanying tables present a list of the Association's investment holdings at the end of the fiscal year, November 30, 1960, and the changes made in these holdings since the last report. Cost prices and approximate market values are shown both in the alphabetical list and in the classified list. The classified list shows government securities, short-term or cash-equivalent and longer term fixed-income securities, and common stocks are listed according to industrial groupings, domestic and foreign.

A comparison of the results over the past year shows that total investments at cost increased from $243,268 in 1959 to $255,418 in 1960, an increase of $12,150. The market value of our holdings over the same period increased from $291,506 to $299,891, an increase of $8,385. The totals include a varying amount of funds representing foundation grants for specific purposes—funds which were invested in short-term governments while projects were getting under way and until withdrawals were called for. These figures are taken into account in reconciling totals reported in the 1959 Finance Committee Report as in the following summary.

COMPARISON OF SECURITY HOLDINGS 1959 AND 1960

Investments		1959	1960
At cost:	Bonds..............	$175,617	$160,508
	Stocks..............	67,651	94,910
	Total..............	$243,268	$255,418
At market:	Bonds..............	$170,370	$157,911
	Stocks..............	121,136	141,980
	Total..............	$291,506	$299,891

As a source of income, interest, dividends, and profits from sale of securities amounted to over 16 per cent of the total income of the Association. (See Treasurer's Report for further details.)

Sales and purchases of securities made during the year are summarized in the accompanying table. It will be noted that the proceeds of the sales of stocks, $26,693, and the net liquidation of U.S. securities to the amount of $15,000 were invested in common stocks totaling $41,560. This shift, plus appreciation in the market value of stocks, increased the proportion of stock-to-bond holdings. The ratio of stocks to total holdings increased, at cost, from 28 per cent in 1959 to 37 per cent in 1960, or, at market, from 33 per cent to 47 per cent, and

LIST OF SECURITIES HELD BY THE ASSOCIATION

Stocks

Number of Shares of Stock	Issue	Cost	Approximate Market Value 11/30/60
100	Abbott Laboratories......................	$ 6,133	$ 5,600
150	American Potash and Chemical Corp..........	6,284	5,700
200	Castle and Cooke..........................	7,200	6,600
400	Central and South West Corp..............	2,802	14,800
150	Chain Belt Co.............................	6,621	7,050
50	Continental Illinois National Bank and Trust Co.	6,619	5,950
100	Deere and Co..............................	4,240	5,100
100	Farbenfabrik—Bayer, AG...................	6,822	9,300
196	Gulf Oil Corp.............................	1,390	5,880
131	Houston Lighting and Power Co............	1,625	11,528
200	International Nickel Co. of Canada..........	7,822	11,200
214	Monsanto Chemical Co.....................	7,756	9,630
100	Motorola.................................	6,746	7,000
110	Peoples Gas Light and Coke Co.............	3,562	6,930
100	Siemens & Halske.........................	5,519	7,800
150	Socony Mobil Oil Co......................	3,882	5,700
100	Standard Oil of Indiana...................	3,650	4,400
110	Wells Fargo American Trust Bank..........	4,261	6,490
50	Zenith Radio Corp........................	1,977	5,050
		$ 94,911	$141,708

Bonds

Par Amount	Issue	Cost	Approximate Market Value 11/30/60
$50,000	U.S. Certif. of Ind., 3½%, "C-1961," due 8/1/61	$ 50,138	$ 50,000
20,000	U.S. Treas. Notes, 4% "D-1962," due 2/15/62.	20,003	20,000
20,000	U.S. Treas. Bonds, 2½%, due 8/15/63........	20,091	19,600
5,000	U.S. Treas. Notes, 3½%, due 5/15/64........	5,000	5,000
8,000	U.S. Treas. Notes, 1½%, due 10/1/64........	8,000	7,440
50,000	U.S. Treas. Bonds, 3½%, due 5/15/66........	50,000	50,000
7,000	U.S. Treas. Bonds, 2½%, due 12/15/72-67.....	7,276	6,020
	Bonds...............................	$160,508	$158,060
	Stocks..............................	94,911	141,708
	Total...............................	$255,419	$299,768

if we exclude outside funds, these ratios are, at cost, 47 per cent to 57 per cent, and, at market, 63 per cent to 69 per cent.

Several formal meetings of the members of this Committee were held during the year for the purpose of reviewing our holdings and making shifts dictated by market conditions and maturing bond issues. The members of this Committee are constantly in touch and have been able to follow closely the composition of our portfolio in relation to the market.

Respectfully submitted,

C. WELLS FARNHAM, *Chairman*
CORLISS D. ANDERSON
JAMES WASHINGTON BELL

SUMMARY OF SECURITIES PURCHASED AND SOLD
YEAR ENDED NOVEMBER 30, 1960
SALES

	Shares or Par Value	Cost	Proceeds	Gain or Loss*
Stocks—				
Columbia Broadcasting System..	106	$ 2,682.20	$ 4,082.78	$ 1,400.58
Fansteel Metallurgical Corp.....	106	4,680.04	7,324.00	2,643.96
Pepsi Cola Co.—Common......	200	4,962.50	8,735.18	3,772.68
Zenith Radio Corp.............	50	1,976.82	5,892.08	3,915.26
Siemens & Halske.............	rights	—	624.05	624.05
Miscellaneous rights...........		—	35.02	35.02
		$14,301.56	$26,693.11	$12,391.55
Bonds—				
U.S. Treasury Notes, 4¾, 8/15/60.	$40,000.00	$40,152.03	$40,004.75	$ 147.28*

EXCHANGES

Par Value			Cost
$ 5,000.00	U.S. Treasury Bonds, 2½, 11/15/61............	U.S. Treasury Notes, 3¾, 5/15/64.............	$ 5,000.00
8,000.00	U.S. Treasury Bonds, 2⅝, 4/1/80.............	U.S. Treasury Notes, 1½, 10/1/64.............	8,000.00
50,000.00	U.S. Certif. of Ind., 4¾, 11/15/60............	U.S. Treasury Bonds, 3¾, 5/15/66.............	50,000 00
25,000.00	U.S. Treasury Notes, 4¾, 8/15/60.............	U.S. Certif. of Ind., 3½, 8/1/61.............	25,095.02

PURCHASES

	Shares or Par Value	Cost
Bonds—		
U.S. Certif. of Ind., 3½, 8/1/61...........	$25,000.00	$25,042.66
Stocks—		
Continental Ill. Nat. Bank & Trust Co....	50	$ 6,618.58
American Potash & Chemical Corp........	150	6,284.25
Farbenfabrik—Bayer, AG...............	100	6,822.28
Standard Oil Co.—Indiana..............	100	3,649.56
Deere & Co............................	100	4,240.00
Castle & Cooke........................	200	7,200.00
Motorola..............................	100	6,745.70
		$41,560.37

* Denotes red.

	Par or Shares	Market Price	Market Value	% of Value	Approximate Income	Cost
Cash Equivalent						
U.S. Treas. 4, 2/15/62..........	$20,000	100	$ 20,000		$ 800	$ 20,003
U.S. Treas. 2½, 8/15/63.........	20,000	98	19,600		500	20,091
U.S. Treas. 1½, 10/1/64.........	8,000	93	7,440		120	8,000
U.S. Treas. 3¾, 5/15/64.........	5,000	100	5,000		175	5,000
			$ 52,040		$1,595	
Bonds						
U.S. Treas. 3¾, 5/15/66.........	$50,000	100	$ 50,000		$1,875	$ 50,000
U.S. Treas. 2½, 12/15/72-67......	7,000	86	6,020		175	7,276
			$ 56,020		$2,050	
Total fixed-income securities...			$108,060	43.3	$3,645	
Common Stocks						
Utilities						
Central & South West...........	400	37	$ 14,800		$ 384	$ 2,802
Houston Lighting & Power......	131	88	11,528		210	1,625
Peoples Gas...................	110	63	6,930		253	3,562
			$ 33,258	23.5	$ 847	
Financial						
Continental Illinois National Bank	50	119	$ 5,950		$ 176	$ 6,619
Wells Fargo American Trust Bank	110	59	6,490		200	4,261
			$ 12,440	8.8	$ 376	
Machinery and Construction						
Chain Belt.....................	150	47	$ 7,050		$ 367	$ 6,621
Deere & Co...................	100	51	5,100		200	4,240
			$ 12,150	8.6	$ 567	
Mining and Metals						
International Nickel	200	56	$ 11,200	7.9	$ 300	$ 7,822
Oil and Gas						
Gulf Oil......................	196	30	$ 5,880		$ 196	$ 1,390
Socony Mobil..................	150	38	5,700		300	3,882
Standard Oil of Indiana.........	100	44	4,400		190	3,650
			$ 15,980	11.3	$ 686	
Chemicals and Drugs						
Abbott Laboratories...........	100	56	$ 5,600		190	$ 6,133
Monsanto Chemical.............	214	45	9,630		214	7,755
American Potash...............	150	38	5,700		180	6,284
			$ 20,930	14.8	$ 584	
Electrical Products						
Zenith Radio..................	50	101	$ 5,050		$ 130	$ 1,977
Motorola.....................	100	70	7,000		100	6,746
			$ 12,050	8.5	$ 230	
Miscellaneous						
Castle & Cooke.................	200	33	$ 6,600	4.7	$ 200	$ 7,200
Foreign						
Bayer, A. G...................	100	93	$ 9,300		$ 125	$ 6,822
Siemens & Halske..............	100	78	7,800		125	5,519
			$ 17,100	12.1	$ 250	
Total common stocks.........			$141,708	56.7	$4,040	
Temporary operating fund.....	$50,000	100	50,000			$ 50,138
Total securities..............			$299,768*	100.0	$7,685	$255,419*

* Includes about $90,000 of foundation funds.

REPORT OF THE AUDITOR

December 20, 1960

Executive Committee
American Economic Association
Evanston, Illinois

DEAR SIRS:

In accordance with instructions we have examined the accounts and related records of the American Economic Association for the year ended November 30, 1960, and now submit our report thereon together with the following exhibits:

Statement of Financial Position—
November 30, 1960   Exhibit 1

Statement of Income and Expense
for Year Ended November 30, 1960   Exhibit 2

Special Funds—Year Ended
November 30, 1960   Exhibit 3

Results from Operations

Net income for the year ended November 30, 1960, was $26,616 compared with net income of $9,785 for the year ended November 30, 1959, as shown in the following summary:

Particulars	Year Ended November 30 1960	1959	Increase Decrease*
Income:			
Dues	$ 59,208	$ 55,214	$ 3,994
Interest and dividends (net)	10,153	6,891	3,262
Profit on sale of securities (net)	12,392	7,469	4,923
Miscellaneous income	154	357	203*
Total income	$ 81,907	$ 69,931	$ 11,976
Expense:			
Publication expense	$ 76,902	$ 70,285	$ 6,617
Less—Publication income	48,515	40,525	7,990
Net publication expense	$ 28,387	$ 29,760	$ 1,373*
Administrative and other operating expense	26,404	29,536	3,132*
Appropriations	500	850	350*
Total expense	$ 55,291	$ 60,146	$ 4,855*
Net income	$ 26,616	$ 9,785	$ 16,831

* Denotes red.

The $3,994 increase in dues reflects the increase in membership as reported by the secretary:

Classification	Number of Members at November 30	
	1960	1959
Regular	9,400	8,910
Junior	1,056	890
Family	150	147
Life	136	117
Honorary	14	14
Complimentary	75	81
Total	10,831	10,159

Interest on bonds owned was accounted for in accordance with stated rates; dividends received on stocks were compared with amounts reported in published records of dividends paid.

Net publication expense, as shown in the following summary, amounted to $28,387 for the current year compared with $29,760 for the preceding year:

Particulars	Year Ended November 30		Budgetary Estimates for Year 1960
	1960	1959	
Expenses:			
Printing of—			
Review	$ 39,429	$ 37,521	$ 42,000
Proceedings	18,915	16,780	16,000
Editors honorarium	5,875	4,500	5,875
Payments to contributors	2,866	1,788	2,600
Editorial clerical salaries	9,292	8,287	7,225
Editorial supplies and expense	525	1,409	800
Total expenses	$ 76,902	$ 70,285	
Less—Income:			
Subscriptions, other than members	$ 23,360	$ 22,272	
Sales of copies	3,456	3,305	
Advertising	20,976	14,948	
Republications	723		
Total income	$ 48,515	$ 40,525	
Net publication expense	$ 28,387	$ 29,760	

Billings for the December, 1960, issue of the *Review* and reprints had not been made by the publishers at the time of our examination. The publishers estimate the cost of the *Review* printings and reprints at $10,265; this amount is included in the year's expenses.

Financial Position

Financial position of the Association at November 30, 1960, and 1959 is set forth in the following summary:

Assets	November 30		Increase Decrease*
	1960	1959	
Cash on deposit and on hand	$ 16,094	$ 14,552	$ 1,542
Receivables (net)	8,155	8,552	397*
Advance to Foundation Project Chairman	2,300		2,300
Prepaid expenses	646	623	23
Equipment, furniture and fixtures (net)	2,168	3,031	863*
Investments at cost—			
Bonds	160,508	175,617	15,109*
Stocks	94,910	67,651	27,259
	$284,781	$270,026	$ 14,755

Liabilities, Funds and Surplus

Accounts payable	$ 12,067	$ 11,893	$ 174
Deferred income	12,803	14,430	1,627*
Funds	89,023	102,331	13,308*
Life membership	14,100	12,300	1,800
Unappropriated surplus—			
Balance at beginning of year	129,072	118,987	10,085
Net income for year	26,616	9,785	16,831
Life memberships transferred to surplus	1,100	300	800
	$284,781	$270,026	$ 14,755

* Denotes red.

Cash on deposit was satisfactorily reconciled with balances confirmed directly to us by the depositories.

The receivables of the Association were not confirmed by correspondence with debtors. Based upon the Association's past experience the reserve for doubtful accounts appears to be adequate to cover normal losses.

Changes in the investment account were verified by the examination of brokers invoices and other supporting data. Securities held at November 30, 1960, were confirmed directly to us by the State Bank and Trust Company of Evanston, Illinois, custodian for the Association.

Insofar as we are able to ascertain, all liabilities of the Association at November 30, 1960, are reflected in the accompanying statement of financial position, and the Secretary has represented to us that to the best of his knowledge all liabilities are disclosed.

Funds

Exhibit 3 shows a net decrease of $13,307 in funds available for specific projects to $89,023 at November 30, 1960. The receipts and expenditures during the year for the various funds and grants are detailed in Exhibit 3.

We express our appreciation for the courtesies and co-operation extended to our representatives during the course of the examination.

Very truly yours,

DAVID HIMMELBLAU & Co.
Certified Public Accountants

Assets			
CURRENT ASSETS:			
Cash on deposit and on hand—			
State Bank and Trust Company, Evanston	$ 9,043.67		
National Bank of Commerce of Chicago	6,990.39		
Petty cash	60.00	$ 16,094.06	
Receivables—			
Review advertising	$ 4,560.00		
Accrued interest and dividends	1,151.34		
Publication sales, etc.	2,447.44		
Membership dues	67.50		
	$ 8,226.28		
Less—Reserve for doubtful accounts	71.30	8,154.98	
Advances to Foundation Project Chairman (unexpended funds in hands of chairman of cumulative index project)		2,300.00	
Inventory of stamps and envelopes		573.32	
Unexpired insurance		72.60	
Total current assets		$ 27,194.96	
INVESTMENTS (at cost):			
Bonds (market value—$157,911.50)	$160,507.78		
Stocks (market value—$141,980.13)	94,909.57	255,417.35	
EQUIPMENT, FURNITURE AND FIXTURES (less accumulated depreciation)		2,168.26	
		$284,780.57	

Liabilities, Funds and Surplus			
CURRENT LIABILITIES:			
Accounts payable	$ 11,666.50		
Due to International Economic Association	400.00	$ 12,066.50	
DEFERRED INCOME:			
Prepaid subscriptions	$ 9,640.22		
Prepaid dues	3,163.00	12,803.22	
SPECIAL FUNDS (Exhibit 3)		89,022.90	
LIFE MEMBERSHIPS AND SURPLUS:			
Life memberships		$ 14,100.00	
Unappropriated surplus—			
Balance November 30, 1959	$129,072.35		
Net income for year ended November 30, 1960 (Exhibit 2)	26,615.60		
Life memberships transferred to surplus	1,100.00	156,787.95	170,887.95
			$284,780.57

Executive Committee
American Economic Association

AUDITORS' OPINION

In our opinion, the accompanying financial statements present fairly the financial position of the American Economic Association at November 30, 1960 and the results of its operations for the year ended that date, in conformity with generally accepted accounting principles applied on a basis consistent with that of the preceding year.

Our examination was made in accordance with generally accepted auditing standards and included such tests of the accounting records and other auditing procedures as we considered necessary in the circumstances.

DAVID HIMMELBLAU & CO.,
Certified Public Accountants

Chicago, Illinois
December 15, 1960

EXHIBIT 2

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF INCOME AND EXPENSE
FOR THE YEAR ENDED NOVEMBER 30, 1960

Particulars		Amount	
INCOME:			
Dues—			
Regular, junior and family members....................		$56,724.86	
Subscribing and contributing members..................		2,483.00	$59,207.86
Investments—			
Interest on bonds......................................		$ 6,692.72	
Dividends...		3,772.44	
		$10,465.16	
Less custodian fees....................................		312.39	10,152.77
Gain on sale of securities...............................			12,391.55
Miscellaneous income..................................			154.50
Total income....................................			$81,906.68
EXPENSE:			
Administrative and other expense—			
Secretary's salary..........................		$ 5,875.00	
Office salaries.............................		22,171.10	
Addressing service income less expense.......		8,521.05*	
Stationery and printing....................		2,369.06	
Postage...................................		1,600.86	
Insurance.................................		169.26	
Executive committee expense...............		2,172.69	
Other committee expense..................		822.20	
Annual meeting—income less expense........		5,124.87*	
Annuity payments..........................		942.92	
Social security taxes.......................		892.25	
Provision for depreciation..................		862.99	
Telephone and telegraph...................		302.46	
International Economic Association..........		400.00	
Office supplies............................		323.68	
Miscellaneous expense (net).................		1,145.58	$26,404.13
Publication expense—			
Printing of—			
Review...............................		$39,429.30	
Proceedings..........................		18,914.77	
Editorial honorarium......................		5,875.00	
Payments to contributors..................		2,866.29	
Editorial clerical salaries..................		9,291.41	
Editorial supplies and expense.............		524.95	
		$76,901.72	

Less—Publication income:				
Subscriptions other than members.	$23,359.60			
Sales of copies.................	3,455.93			
Advertising....................	20,976.22			
Republications................	723.02	48,514.77	28,386.95	54,791.08
				$27,115.60
LESS APPROPRIATIONS:				
Committee on Professional Standards and Ethics........................				500.00
Net income for year (Exhibit 1)...................................				$26,615.60

* Denotes red.

EXHIBIT 3

Fund	Balance November 30, 1959	Received	Expended	Balance November 30, 1960
Carnegie Corporation of New York grant for travel expenses of delegates to international meetings...	$ 6,662.15		$ 4,123.51	$ 2,538.64
Rockefeller Foundation grant for survey articles on recent developments.........................	1,775.00	$ 3,100.00	500.00	4,375.00
The Ford Foundation grants—				
Preparation of a cumulative index of economic journals..........	550.60*	14,928.00	13,446.38	931.02
Preparation of a special register of economists....................	1,841.11		5.61	1,835.50
Translation of books and articles of major significance into the English language.................	25,000.00	—	—	25,000.00
Preparation and publication of articles surveying economic research in foreign countries.....	40,000.00		914.93	39,085.07
Survey study of economic textbooks in secondary schools.....	24,630.91		11,950.00	12,680.91
Committee on Publication and Research........................	381.76		28.59	353.17
Committee on Economic Education.	431.66		323.86	107.80
Committee on *Papers and Proceedings*	250.00	—	—	250.00
Committee on Professional Standards and Ethics......................	(1)	500.00		500.00
Asiatic Foundation...............	1,854.65		611.86	1,242.79
Sundry...........................	53.75	75.00	5.75	123.00
Total (Exhibit 1)............	$102,330.39	$18,603.00	$31,910.49	$89,022.90

(1) Represents appropriation from the current year's operations (see Exhibit 2).

* Denotes red.

REPORT OF THE MANAGING EDITOR FOR THE
YEAR ENDING DECEMBER 1960

The number of manuscripts received during 1960 was approximately the same as in 1959, although the proportion of articles submitted was somewhat lower. Table 1 gives comparative figures for the past six years.

TABLE 1
MANUSCRIPTS SUBMITTED 1955–60

	1960	1959	1958	1957	1956	1955
Manuscripts received......	276	279	242	215	242	245
Articles...............	158	180	151	141	153	149
Communications.......	118	99	91	74	89	96
Percentage of articles accepted...............	16	14	17	19	18	17

Table 2 provides the breakdown of the volume's contents between articles, review articles, communications, book reviews, etc. A somewhat greater proportion of the volume was devoted to articles than in recent years, but this outcome in no way reflects a change in policy. One survey article was included in the 1960 volume, as was also the case in 1959.

TABLE 2
SUMMARY OF CONTENTS, 1958–60

	1960		1959		1958	
	Number	Pages	Number	Pages	Number	Pages
Leading articles..........	22	478	20	408	22	428
Review articles...........	3	67	5	74	4	50
Communications:						
Original..............	9	49	7	46	6	30
Comments and replies...	14	36	16	94	12	79
Book reviews............	196	342	205	345	181	327
Classified lists:						
New books............	—	62	—	63	—	63
Periodical articles.......	—	61	—	69	—	63
Dissertations..........	—	28	—	30	—	32
Notes...................	—	42	—	42	—	47
		1,165*		1,171*		1,119*

* Plus some blank pages.

A third survey article will appear in the March, 1961, issue; and it is expected that a fourth will be ready for publication in the June issue. Two others are now scheduled for 1962; and the remaining two of the series will shortly be

assigned by the Board of Editors. This series of eight survey articles is made possible by a grant provided by the Rockefeller Foundation.

Table 3 summarizes the subject-matter distribution of articles, review articles, and communications for the past five years; the figures in parentheses give the distribution for 1960 only. The most interesting figures are those for leading articles plus original communications, since to some extent these figures indicate the areas in which the most work of publishable quality and of broad interest to economists is being done. The six fields showing the highest concentration for the last five years are as follows: Income and Employment Theory (21), Price and Allocation Theory (16), Economic Development (15), International Economics (15), Public Finance (13), and Money and Banking (9).

TABLE 3

SUBJECT-MATTER DISTRIBUTION: ARTICLES AND COMMUNICATIONS, 1956–60 AND 1960

	Articles	Review Articles	Original Communications	Comments: Replies	Totals
General economics........	7	—	2 (2)	—	9 (2)
Price theory.............	14 (5)	3	2	6 (2)	25 (7)
Income theory...........	16 (2)	2	5	11	34 (2)
History economic thought.	4 (1)	4 (1)	1	2 (2)	11 (4)
Economic development....	13 (4)	—	2 (1)	15 (6)	30 (11)
Social accounting........	2	2	1	—	5
Economic systems........	3	2	—	—	5
Business fluctuations......	5 (2)	2	1	2 (1)	10 (3)
Money and banking......	5 (2)	1 (1)	4	2	12 (3)
Public finance...........	6 (1)	3 (1)	7 (2)	7	23 (4)
International economics...	14 (2)	1	1 (1)	2 (2)	18 (5)
Business finance.........	2	2	—	3	7
Business organization.....	3 (2)	—	—	—	3 (2)
Industrial organization....	4	—	2	4	10
Land economics..........	3	1	1	2	7
Labor economics.........	4	1	1 (1)	4	10 (1)
Population; welfare.......	2	—	—	—	2
Unclassified.............	1 (1)	—	4 (2)	4 (1)	9 (4)
	108 (22)	24 (3)	34 (9)	64 (14)	230 (48)

NOTE: The 1956–60 figure is followed in each case by the 1960 figure in parentheses.

Table 4 presents the expenditures in 1960 for the four regular issues of the *Review* in comparison with the estimated budget and with the actual expenditures in 1959. The budget figure for 1960 did not include the estimated expense of publishing one survey article, but the actual expenditure figure for the year does include this expense—approximately covered by an allocation of $1,725 from the Rockefeller Foundation grant. The budget for 1960 was based on an anticipated volume of 1,430 pages, including advertising (or about 1,150 pages of text) and an average number of copies of 14,750. Actually the number of pages in the volume (exclusive of the survey article of 49 pages) was 1,116 pages net, or 1,343 gross—which is the main reason that we were able to keep substantially within our budget.

TABLE 4

ACTUAL AND BUDGETED EXPENDITURES

	Budget 1960	Actual 1960	Actual 1959
Printing and mailing........	$42,000	$39,660.97	$37,905.09*
Editor's salary..............	4,500	6,000.00	4,500.00
Editorial assistance.........	8,600	9,006.52	8,373.88
Legal service..............	—	—	40.00
Supplies...................	800	634.44	785.04
Contributors..............	2,600	2,918.25	2,923.75
	$58,500	$58,220.18	$54,527.76*

* Corrected from 1959 report.

Table 5 gives detailed information about printing cost by issues. The number of copies printed in 1959 averaged 14,125; in 1958, 12,975; 1957, 12,400. The successive increases are of course mainly related to the increases in membership and subscriptions.

TABLE 5

COPIES PRINTED, SIZE AND COST OF PRINTING, 1960

	Copies Printed	PAGES		Issue	Reprints	Cost Including Reprints
		Net	Gross			
March.....	14,800	294	360	$10,340.17	$ 93.67	$10,433.84
June.......	14,800	259	304	8,521.26	89.61	8,610.87
September..	15,000	338	384	10,465.23	86.03	10,551.26
December..	15,400	276	344	9,975.00†	90.00†	10,065.00
		1,167*	1,392	$39,301.66	$359.31	$39,660.97

* Includes 2 blank pages.
† Estimated.

The estimated costs for the coming year are presented in Table 6, based on a volume of 1,420 pages, including advertising (or about 1,150 pages of text, exclusive of survey articles) and an average number of copies of 16,000. Printing costs reflect an anticipated increase of 10 per cent in the rates charged by our present printer. Anticipated cost of survey articles is not included, since this cost will be substantially covered by the Rockefeller Foundation grant.

TABLE 6

RECOMMENDED BUDGET FOR 1961

Printing (including paper, postage, reprints, etc.)	$45,200
Editor's salary	6,000
Editorial assistance	9,300
Supplies	800
Contributors	2,500
	$63,800

During the year I have had the advice and assistance of the following foreign correspondents—who have been particularly helpful with regard to the selection of foreign books for listing and review:

Isaac Kerstenetzky (Brazil)	Paolo Sylos Labini (Italy)
Maurice Flamant (France)	Oscar Soberón (Mexico)
Erich Schneider (Germany)	P. J. Verdoorn (Netherlands)

Bent Hansen (Sweden)

Two members of the Board of Editors complete their three-year terms of office at this time: O. H. Brownlee and Kermit Gordon. I wish to express appreciation of the generous expenditure of time they have made in the interests of the *Review* and the Association. I nominate for three-year terms beginning in 1960: A. E. Kahn and J. A. Pechman.

During the year I have frequently sought the aid of members of the profession in addition to the members of the editorial board—partly to relieve the latter of what would otherwise be an impossibly heavy burden and partly to obtain advice of specialists in particular areas not represented on the Board. The following have assisted in this way:

I. Adelman	K. E. Boulding	A. O. Hirschman	A. G. Papandreou
A. A. Alchian	J. M. Buchanan	S. Hoos	A. Rees
J. W. Angell	L. V. Chandler	D. G. Johnson	E. Rolph
G. L. Bach	W. W. Cochrane	N. M. Kaplan	W. S. Salant
J. Bain	A. Eckstein	C. P. Kindle-	T. C. Schelling
P. Baran	R. Eisner	berger	W. L. Smith
F. M. Bator	W. Fellner	A. P. Lerner	P. Sweezy
W. J. Baumol	L. Fisher	J. Margolis	W. Vickrey
A. Becker	M. Friedman	G. Meier	T. M. Whitin
P. Bell	I. Friend	R. F. Mikesell	
A. Bergson	W. Galenson	F. W. Notestein	

Respectfully submitted,

B. F. HALEY, *Managing Editor*

REPORT OF THE COMMITTEE ON RESEARCH
AND PUBLICATIONS

This Committee was reconstituted in spring 1960 by President Schultz under its present Chairman. It was asked to take over certain continuing responsibilities initiated under its earlier chairman, John Perry Miller, and, as well, to report in December, 1960, its recommendations concerning the future of the Committee.

Of activities previously initiated, one is the index of economic periodicals, which Professor Miller agreed to bring to conclusion, and on which he is presumably reporting directly.

Another earlier project is a volume of *Readings in Economic Development*, under the editorship of Max Millikan and Henry Bruton. Although the project has been delayed by numerous other assignments of its editors, it is our understanding that they are now making progress and that the final stages of their editorial work should shortly be concluded.

A third continuing project is the translation and publication of a limited number of significant foreign-language books, to be partially financed by a grant of $25,000 from the Ford Foundation. The translations which the Committee has so far agreed to attempt to commission are (*a*) Pareto's *Manuel* (French), with appropriate cross-references to the *Manuale* (Italian), the *Cours*, and other writings of Pareto; (*b*) Von Thünen's *Der Isolierte Staat* and perhaps other writings; and (*c*) Dahmen's *Svensk Industriell Företagarverksamhet*. Other works have been suggested and are being investigated; but the Committee has agreed to concentrate first attention on the works named above. No specific project has yet reached the stage at which the Committee is prepared to recommend action. As an alternative to the translation of some important foreign books, the Committee is investigating instead the encouragement of lengthy review articles which would present to English-speaking scholars a full summary of the content of such works. The Chairman has been in touch with both Professor Haley and Professor Hildebrand regarding this proposal, and any recommendation to proceed along this line would require careful coordination with these other arms of the Association.

The Committee on Research and Publications might be abandoned once these previously-initiated projects were brought to conclusion. This, however, is not our recommendation. After consulting numerous of our colleagues in the profession, it is our judgment that the "Readings Series" should not automatically be terminated with the publication of the *Readings in Economic Development*. We have, for example, been investigating carefully the advisability of a volume of *Readings in Welfare Economics*. Such a project has received widespread support, not only from specialists in this field, but also from teachers in many leading universities. Particularly if welfare economics is defined rather broadly, we believe that the project has sufficient merit to support our further study of it.

Quite apart from our ultimate recommendation on this particular volume, however, we believe, and we think the profession believes, that the "Readings Series" has made and can still make an important contribution. To be sure, commercial ventures may now be expected to fill much of the need which led to the initiation of the series; but where commercial initiative is lacking or the market insufficient to attract it, the Association may still perform a useful function. This may also be the case because the Association sponsorship can often enlist better editorial talent and the advice and assistance of a wider circle of distinguished experts than could any commercial venture.

Thus, we recommend that the Committee be maintained in existence in order to receive and evaluate suggestions for further volumes of *Readings* and to be responsible for recommending and administering the publication of possible additional items in this series.

In addition, we believe that other suggestions will continue to be received by the Association for further activities in the areas of research and publications. A committee should be available to which such suggestions can be referred. Most of these can be politely buried by the Committee; but it can also be a source of support and an organ of action for such good ideas as may occasionally come forward. Knowing how busy members of such a committee will always be, the Association can be confident that the Committee will never be in the position of looking for projects merely to justify its existence.

GARDNER ACKLEY, *Chairman*
JAMES WASHINGTON BELL
ROBERT AARON GORDON
WILLIAM C. NICHOLLS
WILLARD L. THORP

REPORT OF THE COMMITTEE ON ECONOMIC EDUCATION

1. The High School Textbook project is nearing completion. The three underlying reports are in the hands of the Director of the project (Paul R. Olson); he has prepared a paper based on these reports, to be read at the session on Economic Education to be held during the present meetings; and the covering statement by the Director will soon be ready and the complete report will then be submitted to the Committee on Economic Education. Following instructions, the Committee will make its recommendations for distribution of the Final Report, at the March, 1961, meeting of the Executive Committee.

2. The American Council of Learned Societies, through its Committee on Secondary Education, is arranging for a series of statements by representatives of the several social sciences on the purposes of their respective disciplines in the curricula of the secondary schools. The statements will be published. The Chairman of the Committee on Economic Education is preparing the statement on economics.

3. Following the report and recommendation made by the Committee in March, 1960, and the action taken at that time by the Executive Committee, the National Task Force on Economic Education was established. The Committee provided the panel of names from which the membership of the Task Force was chosen by the President of the AEA. Henceforth, the Task Force will make its own reports directly to the Executive Committee.

4. The Committee continues its active relationship with the National Council for the Social Studies and with the Joint Council on Economic Education. Representatives of the Committee attended and participated in the work of the San Diego Conference sponsored by the National Commission on Teacher Education and Professional Standards, and the National Education Association.

5. The Committee recommends that one session of the annual meetings to be held a year hence be devoted to the teaching of economics at the college level.

BEN W. LEWIS, *Chairman*

REPORT OF THE *AD HOC* COMMITTEE
ON ECONOMIC ABSTRACTS

Following action of the Executive Committee in March, President Schultz appointed an *ad hoc* committee to meet with a group of journal editors and to report on the possibility and value of regular publications of abstracts of articles in economic journals. The appointed committee (except for J. J. Spengler) met with an invited group of journal editors in New York on September 10. Those present were:

Members of the committee: Gardner Ackley, William H. Nicholls, R. A. Gordon.

Editors: Mary Jean Bowman, *Journal of Political Economy;* Bernard Haley, *American Economic Review;* G. T. Schwenning, *Southern Economic Journal;* Arthur Smithies, *Quarterly Journal of Economics;* Robert Strotz, *Econometrica.*

From the Ford Foundation: Richard Ruggles and (for part of the meeting) Kermit Gordon.

S. E. Harris, editor of the *Review of Economics and Statistics,* was invited but was unable to attend. Professor Spengler presented his views in a letter to the Chairman of the Committee in advance of the meeting.

An agenda consisting of six questions was circulated in advance, and discussion at the meeting addressed itself almost entirely to these questions. The remainder of this report will, therefore, summarize the group's views on each of these questions.

1. *How useful would the regular publication of abstracts of articles in leading journals be?*

The view was unanimous that regular publication of abstracts would be very useful. A minority thought that this would be so even if coverage were limited to a few leading journals. There was general agreement that the usefulness of the abstracts would be greater if the coverage were to include the leading foreign as well as the leading U.S., Canadian, and U.K. journals. Assuming fairly broad coverage, regular publication of abstracts would perform a very useful function.

2. *Assuming that in general abstracts are useful, is this the appropriate time to try to launch regular publication of abstracts? And if so, what range of journals should be covered?*

It was believed that now is a good time to start, and that there would be from the beginning strong international interest in the venture. It may be necessary to begin with a limited list of journals, but the coverage should be international from the start. The initial list of journals might include those which could be abstracted on a nonselective basis (i.e., all articles would be abstracted). It

might be desirable to list articles in the journals not abstracted. In that event the new publication would absorb the periodicals index now published in the *AER*.

3. *Assuming favorable answers to the preceding questions, what would be the most convenient form of publication, and under whose sponsorship?*

The view was unanimous that the publication of the abstracts should be sponsored by the American Economic Association, and a majority felt that all AEA members should automatically receive the abstracts. Sponsorship by an international organization was *not* favored, although it might be desirable to have some sort of international advisory board. The preferred form of publication was as a paper-covered pamphlet to be issued quarterly. It would be desirable to have publication of the abstracts associated closely with the *American Economic Review*. The group recommended that this matter be looked into by the Association in connection with the re-examination of the activities of the editor of the *AER*, which, it was understood, would probably take place in connection with the appointment of a successor to the present editor (whose intention to retire has already been announced).

4. *What would be the most feasible way of having the abstracts prepared? Some of the possibilities are the following. The co-operating journals might require that authors supply abstracts when articles are accepted for publication. The journals themselves might prepare the abstracts. Funds might be raised so that the agency publishing the abstracts could pay to have them prepared.*

Responsibility for preparing the abstracts should be left with the journals to be included. The abstracts would be prepared either by the editors and their staffs or by the authors. Eventually some procedure will be needed to select articles in journals which are not to be completely abstracted, and the foreign-language journals may present some problems at the very beginning.

5. *What, roughly, would be the cost of alternative ways of preparing and publishing the abstracts?*

It was thought that the most effective procedure would be to use a photo-offset process on the basis of typed copy prepared in the office of the editor of the journal of abstracts. Professor Haley was kind enough to offer to secure printing estimates. (Actually, estimates received from printers indicate that linotype printing would be more economical. See Appendix A.) In addition to printing and mailing, costs would include an honorarium for the editor, the salary of a secretary-assistant, and the usual office overhead expense.

6. *Finally, given the group's conclusions as to the value of abstracts to the profession and the costs in time and money of alternative ways of preparing and publishing them, should a favorable or unfavorable recommendation be made to the Executive Committee of the Association? If favorable, what specific recommendations should the group make?*

The group unanimously concluded that the publication of abstracts should

be undertaken by the Association—that the costs of the venture would be outweighed by the potential usefulness of the abstracts to the profession.

Although this recommendation was unanimous, a few of the journal editors present expressed concern that the abstracts might have an unfavorable effect on the circulation of their journals. The majority of the group, however, held to the view that the abstracts would serve to increase the subscription lists of the journals included.

A draft of this report was circulated among all members of the *ad hoc* Committee and among the journal editors attending the meeting and suggested changes have been incorporated in the report. All of the undersigned members of the Committee concur in the above recommendations.

Attached are two appendices, both kindly prepared by Professor Haley. One provides a suggested budget; the other presents two lists of foreign periodicals (other than U.K.) that might be covered.

R. A. Gordon, *Chairman*
Gardner Ackley
William H. Nicholls
Joseph J. Spengler

APPENDIX A

Suggested Budget
(four quarterly issues)

	A. Offset	B. Linotype
Editor	$ 1,000	$1,000
Secretarial	1,000	1,000
Supplies	200	150
Printing*	9,000	5,200
Mailing	275	275
	$11,475	$7,625

* The item for printing is based on an assumed 48-page pamphlet (linotype) or 72-page pamphlet (offset), 15,000 copies, to be mailed with regular issues of the *AER*.

APPENDIX B

Foreign Periodicals (other than U.K.)

First List

Australia—*Economic Record*
Austria—*Zeitschrift für Nationalökonomie*
Canada—*Canadian Journal of Economics and Political Science*
Denmark—*Nationalokonomisk Tidsskrift*
France—*Economie Appliquée Revue d'Economie Politique*
Germany—*Schmollers Jahrbuch Jahrbuch für Nationalökonomie und Statistik*
Italy—*Giornale degli Economisti Metroeconomica*
Sweden—*Economisk Tidskrift*
Switzerland—*Kyklos*

Second List

Germany—*Zeitschrift für die gesamte Staatswissenschaft*
India—*Indian Economic Journal*
Italy—*Economia Internazionale Studi Economici Revista Internazionale de Scienze Economiche e Commerciale*
Japan—*Kyoto University Economic Review Annals of the Hitotsubshi Academy Kobe Economics and Business Review*
Mexico—*El Trimestre Economico*
Norway—*Økonomi Naeringsøkonomisk Forskningsinstitut*
South Africa—*South African Journal of Economics*
Switzerland—*Schweizerische Zeitschrift für Volkswirtschaft und Statistik*

ECONOMIC INTERNSHIPS: PROPOSED BROOKINGS
CONGRESSIONAL FELLOWSHIP PROGRAM

The purpose of this memorandum is to request the support of the American Economic Association for a program which it is hoped will be undertaken by the Brookings Institution to provide three Congressional Fellowships for younger economics faculty members. If the Association decides to support the program, it is expected that the Brookings Institution will submit a request for funds to one of the foundations to get the program under way.

Under the proposed program three fellows would be assigned annually to Congressional committees or to the offices of senators and congressmen where their primary duties would be to work on reports or projects involving economic analysis. Some latitude as to the nature of the work would have to be permitted in order to satisfy the varying needs of the different fellows and legislators, but it would be understood in advance that the greater part of the work would contribute directly to the research and professional competence of the fellows. The three Congressional Fellows would also participate in the regular Brookings seminars and various activities carried on for their other fellowship holders.

The administration of the program would be handled by the Brookings Institution in co-operation with the American Political Science Association. The latter already handles a similar program for political scientists, journalists and law school teachers. To avoid confusion in the minds of congressmen, it would be advisable to use the facilities now in existence for selecting and placing recipients to the greatest extent possible. At the same time, since the professional needs of economists are quite different from those of the other participants, this fact would have to be recognized at every step in the procedure.

Both Dr. Robert Calkins, President of the Brookings Institution, and Dr. Evrom Kirkpatrick, Executive Secretary of the American Political Science Association, have responded most favorably to the suggestion that a program along the above lines be undertaken.

FRANK C. PIERSON

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INSTITUTE OF INTERNATIONAL EDUCATION ADVISORY AND POLICY BOARD

EDITORIAL NOTE: In lieu of a formal report, the Chairman of the Board, Theodore Morgan, has submitted a summary outline of information on the Economics Institutes administered by the Institute of International Education under provisions of a grant from the Ford Foundation.

Origin of the Economics Institutes. In 1956, Lloyd Reynolds and Kermit Gordon, of the Ford Foundation, visited many departments of economics in the United States, inquiring among other things about their problems in connection with the training of increasing numbers of foreign graduate students in economics and agricultural economics.

As a result of their inquires, an informal committee was set up and in 1957 developed successive drafts of a proposal for summer institutes for entering foreign students in economics and agricultural economics. Letters were sent out to a large number of U.S. graduate schools to obtain information on their problems with foreign students, their enrollments, and their suggestions on the proposed institutes. The final draft proposal provided for a trial period of three summer institutes, with instruction centered about equally on economics and English, plus orientation toward the U.S. economy and society. The proposal was accepted by the Ford Foundation in November, 1957, with a grant of $185,000.

The Policy and Advisory Board. In December, 1957, the Executive Committee of the American Economic Association appointed the following people to a Policy and Advisory Board for the Institutes: Rendigs Fels (Vanderbilt University), Carter Goodrich (Columbia University), Michael L. Hoffman (International Bank), D. Gale Johnson (University of Chicago), Irving B. Kravis (University of Pennsylvania), Theodore Morgan, Chairman (University of Wisconsin), and Lorie Tarshis (Stanford University).

The First Three Institutes. Economics Institutes have been held in the summers of 1958, 1959, and 1960. The first was at the University of Wisconsin, the second and third at the University of Colorado. Wyn F. Owen, of the University of Colorado, has been the Director of all three institutes.

The grant has been administered by the Institute of International Education. In these three institutes, 131 students from 35 countries attended, before starting their graduate work in 43 different U.S. universities. In the 1960 institute, 70 applications were sent in. Of these 13 were rejected by the Director and 17 students resigned after acceptance. The remaining 46 enrollees represented 22 countries and 24 U.S. universities.

The Board Meeting of November 4, 1960. The Board voted to carry on the Institutes for two more summers, financing them with the unexpended balance from the grant. It recommended that enrollment be expanded in 1961 to 60-65 fellows; and that students who are weak in both economics and English be excluded from the program, with reasons for rejection being given to the U.S. universities where they are to be enrolled.

THE BUDGET

	Gross Cost	Contributions from the Department of State and Other Sponsors	Net Cost
Direct expenses:			
1958 Institute (36 students)	$35,451	$ 9,244	$ 26,207
1959 Institute (49 students)	45,745	15,411	30,334
1960 Institute (46 students)	46,006*	16,300*	29,706*
Total net cost			$ 86,146*
Administrative overhead . . .			16,000*
Total spent from grant			102,146*
Unexpended			82,944*

* Estimated.

Kermit Gordon, of the Ford Foundation, who attended the meeting, announced the appointment of Holland Hunter (Haverford College) to evaluate the first three Institutes. He will estimate the value of the program as compared to its expense and make recommendations as to what can be done to improve it.

Announcement of the Fourth Session, June 30-August 30, 1961, at University of Colorado, Boulder, Colorado: The Institute is designed for students from foreign countries who are about to begin graduate studies in economics or agricultural economics in the United States. Students who have spent not more than one semester in the United States prior to the Institute, as well as students from overseas, are eligible to apply. The Institute provides an intensive review of microeconomic and macroeconomic theory, offers concentrated training and practice in oral and written English, and supplementary training in mathematics. A general introduction to the United States economy and society is also provided. Instruction is adapted to the needs of individual students on the basis of the results of an initial testing program. Final admission to the Institute will be granted only to those students who can provide proof of admission to graduate work at a university in the United States for the academic year following the Institute.

REPORT OF REPRESENTATIVE TO THE AMERICAN COUNCIL
OF LEARNED SOCIETIES

The annual meeting of the American Council of Learned Societies was held in January, 1960, in New York. It was attended by the delegate from the AEA as a constituent society (the undersigned) and by the Secretary, Professor James W. Bell. The organized Conference of Secretaries is an important feature of the Council structure, its proceedings being reported to the official body, made up of delegates and a Board of Directors. On this occasion, besides the regular business of the meeting, the attendants, with a number of invited guests, were provided with a most interesting program of scholarly papers and an evening lecture on the topic, "The Bible and the Humanities."

During 1960, the Council was engaged in a typically various list of activities —some continuing, others more or less new. Its budget for the fiscal year was slightly under $700,000. In a brief report, only a few of its activities can be mentioned. They are largely conducted by twenty-odd committees of specialists, working with and under the Board of Directors, which met six times during the year, and the President and Chairman. Anyone seriously interested in details of its work may get the annual reports or the monthly *News Letter* on request from the offices at 345 East 46th Street, New York 17. The major news item for the organization was the securing in December of foundation support assuring continuance of its main program for an ensuing decade.

As should be recalled, the ACLS is an organization of and a service agency for the humanities and social sciences, which practically means the professional academic associations not represented by the National Research Council, dealing with natural science interests. When it was founded, in 1919, the Social Science Research Council did not exist, and for some time after its establishment it was not contemplated that a main function of such bodies would be to solicit funds, from foundations or other sources, and use these to support advanced education and research projects. In later years this became a main activity of both councils, but with the development of the SSRC it has largely taken over this function for the major social sciences that are constituents of both—especially the four apart from history, which is considered definitely a humanistic interest. The majority of individuals or projects receiving grants of money from the ACLS are in either the field of history or of English. (The Anthropological Association is a member of all three Councils.) However, the distinctively historical-research and critical or social-philosophical aspects of economics and the others are duly recognized, notably in the award of the annual ACLS prizes of $10,000 which have been made for the past three years (and will be continued for at least two more).

Rather unfortunately, perhaps—particularly as regards the ACLS and its constituents—the securing and disbursement of funds for fellowships, grants-in-aid of research and special projects tend to pre-empt the attention of the members of constituent societies, as well as the limited public which cares about scholarly matters. The main service of the Council to economics and kindred

interests (apart from the grants and prizes) relates to broad fields of scholarship and research, notably in general education, making materials available, publication of research, and relations with governmental organs and academic or scholarly bodies abroad. The occasion for its organization was to provide American representation in the International Union of Academies, formed at the close of World War I, and this field of activity has expanded in recent years. A main concern of the Council is to increase American knowledge of other nations and cultures, which has become so important for this country's new responsibilities in world affairs. In recent years the Council activities have more and more become joint responsibilities with the SSRC, beginning with the selection of Fulbright appointees, handled by a Conference Board of the three reseach councils. Relations with the USSR and Slavic nations have taken on major importance. A Joint Committee of the ACLS and SSRC has prepared and published the important *Digest* of the Soviet press, and in 1960 published a three-volume report on a review of Russian studies in the United States. The ACLS conducted negotiations with the Academy of Sciences of the USSR leading to an agreement for the exchange of scholars between that country and the U.S. Foundation grants were secured to meet the expenses in this country, and a list was prepared and submitted of suitable and interested American scholars; but at the end of the year details of the arrangement had not been settled.

Other joint activities of the two councils were programs for Asian and Latin-American area research. During the year, to this were added new committees on contemporary China and Africa, and foundation grants were secured for their support. The ACLS has continued its concern for providing personnel trained in languages not generally taught in American universities. For example, of forty-two projects in the Uralic-Altaic field, over a third have been completed, and programs are being conducted in Southeast Asian and in Oriental languages. The Council has promoted a conference to discuss improvement in the social studies teaching in secondary schools. Two other important activities are arranging and securing support for American participation in international scholarly conferences held abroad, and for holding such conferences in this country and bringing foreign representatives to them. The Council has promoted important reference works such as the *Dictionary of American Biography*, and arranged for photographic or microfilm reproduction of records in other countries needed for research in this country. As indicated above, this is a "sampling" of activities too numerous and varied for brief summarizing.

FRANK H. KNIGHT

REPORT OF REPRESENTATIVES TO THE SOCIAL SCIENCE
RESEARCH COUNCIL

During 1960 the Social Science Research Council was concerned with a substantial number of problems regarding the development of research in the social sciences which are of especial interest to economists. Among its activities were several conferences, held late in the year, which are likely to have further significant impact on the strengthening of survey research facilities, their availability to individual social scientists, and the preservation and use of economic data. Toward the close of the year the Council also embarked upon a revitalization of its Committee on Mathematics in Social Science Research with a hope that an active new program can be announced by next spring. A notable event was the publication last summer of *Historical Statistics in the United States, Colonial Times to 1957,* issued by the U.S. Bureau of the Census with the aid of the Council's Committee on Historical Statistics under the chairmanship of G. Heberton Evans, Jr.

A number of other committees of the Council were particularly concerned with economic research. The Committee on Agricultural Economics, under the chairmanship of Herman M. Southworth, re-examined several neglected areas of research in its field. One important result was the publication during 1960 of two articles in the *Journal of Farm Economics:* "The Analysis of Changes in Agricultural Supply: Problems and Approaches," by Marc Nerlove and Kenneth L. Bachman, in the August issue; and "Research on the Economics of Technological Change" by Vernon W. Ruttan, in the November issue. It is expected that additional articles will appear in the course of the next two years. The Committee is also currently turning its attention to the formulation of a major research project on the international role of American agriculture, particularly in relation to the underdeveloped countries.

The Committee on Analysis of Economic Census Data, with John Perry Miller as Chairman, continued its efforts to make the economic statistics gathered by the Bureau of the Census of greater usefulness to economists. One of the by-products of this effort during 1960 was the publication by the Bureau of the Census of its Working Paper No. 9, *Historical Comparability of Census of Manufactures Industries, 1929-1958,* by Harold T. Goldstein. One of the Committee's own projects, the study of the location of manufacturing (Victor R. Fuchs) was nearly ready for the press by the end of the year. Work is continuing on studies on the industrial structure of large diversified manufacturing firms (Carl Kaysen), price-cost behavior (Richard and Nancy Ruggles), industrial concentration (Ralph Nelson), the structure of retail and wholesale trade (Richard Holton), and an exploratory study of the validity of currently used industrial classifications (James W. McKie).

Under the guidance of Walter Galenson and Alexander Eckstein, the Joint Committee on Contemporary China (of the Council and the American Council of Learned Societies) held a conference of economists in September to explore the feasibility of stimulating and expanding research on the economic situation

and growth of Communist China, with the expectation that this Committee (in conjunction with the Committee on Economic Growth) may in the course of the coming year launch a major research effort concerning research on the economics of China.

The Committee on Economic Growth, under the chairmanship of Simon Kuznets, during 1960 held the three conferences (on natural resources, on the rate and direction of inventive activity, and on agriculture and economic growth) mentioned in last year's report to the Association. It also completed plans for two conferences to be held in 1961—on the economics of Soviet industrialization and on the economics of Sub-Saharan Africa. A volume growing out of an earlier conference, *Labor Commitment and Social Change in Developing Areas,* edited by Wilbert E. Moore and Arnold S. Feldman, was published by the Council in December. Publications resulting from the Committee's collaborative foreign studies are also beginning to appear. One issued in 1960 was *Danmarks Udenrigshandel, 1874-1958,* by Ole Bus Henriksen and Anders Ølgaard. An *Abstract of Historical Statistics* relating to the United Kingdom, by Phyllis Deane and W. A. Cole, is to be published in 1961 and a monograph by the same authors, *The Course of British Economic Growth,* in 1962. As an outgrowth of an interuniversity summer research seminar planned by the Committee, a volume on *Theories of Economic Growth,* edited by Bert F. Hoselitz, was published in 1960; so with the fifth of a series of essays summarizing the results of Professor Kuznets' comparative studies of economic growth, "Quantitative Aspects of the Economic Growth of Nations: V. Capital Formation Proportions: International Comparisons for Recent Years," which appeared as a supplement to *Economic Development and Cultural Change,* July, 1960.

The Committee on Economic Stability, appointed in 1959 under the chairmanship of R. A. Gordon, during the year formulated a two-year project to review and assess efforts to develop an econometric model of the economy of the United States. It is expected that fifteen to twenty economists will participate, under the direction of Lawrence R. Klein and James S. Duesenberry. Toward the end of the year, the Committee also had under consideration a series of studies on investment behavior in particular industries and a possible series of co-ordinated "country studies" to investigate, within a common analytical framework, the kinds of instability or stability experienced by different countries.

The Committee on the Family and Economic Behavior, concerned especially with bringing about the collaboration of economists and sociologists in formulating models of the economic behavior of households, has been discontinued on the ground that additional work will have to be completed before the Committee's assignment can be achieved. The projects initiated by the Committee on Population Census Monographs reported last year are moving forward somewhat slowly because they are dependent upon the availability of special census tabulations drawn from the 1960 census. More substantial progress is expected within the coming year. This is also true of the efforts of the Committee on Urbanization, noted in last year's report, to formulate a critique of research on the economic as well as the noneconomic aspects of city and urban phenomena. The Committee on National Security Policy Research has con-

tinued its efforts to interest economists in research on the questions with which it is concerned.

Economists have continued to receive assistance under the Council's general fellowship and grant-in-aid programs, as well as under the several "area" research committees appointed jointly by the Council and the American Council of Learned Societies. Sanford A. Mosk was chairman of the Joint Committee on Latin-American Studies until his recent death, Abram Bergson is currently chairman of the Joint Committee on Slavic Studies, and Evsey D. Domar is chairman of the Joint Committee on Slavic and East European Grants. Joseph J. Spengler continues to serve as chairman of the Council's Committee on Problems and Policy, and Gardner Ackley is this year a member of the Council's Executive Committee. In addition to the undersigned, R. A. Gordon and Gardner Ackley were during the past year members of the Board of Directors of the Council from the American Economic Association.

WILLIAM H. NICHOLLS

REPORT OF REPRESENTATIVE TO THE NATIONAL BUREAU OF ECONOMIC RESEARCH

The year 1960 was one of large output for the National Bureau of Economic Research. Eighteen titles were added to its list of publications, twelve reports were in press at the end of the year, and sixteen others were nearing completion. Of particular importance was the review of official price indexes and price statistics undertaken at the request of the Bureau of the Budget. The report of the Price Statistics Review Committee which the Bureau established was submitted to the Bureau of the Budget in December.

The new publications have received general publicity but it may be helpful to list the work in process.

REPORTS IN PRESS, DECEMBER 31, 1960

Trends in Government Financing, by Morris A. Copeland (January, 1961).

Business Cycle Indicators, Volume I, *Contributions to the Analysis of Current Business Conditions;* Volume II, *Basic Data on Cyclical Indicators,* edited by Geoffrey H. Moore (February, 1961).

Public Finances: Needs, Sources, and Utilization, by a Special Conference of the Universities-National Bureau Committee for Economic Research (February, 1961).

Real Wages in Manufacturing, 1890-1914, by Albert Rees (March, 1961).

Output, Input, and Productivity Measurement, by the Conference on Research in Income and Wealth (March, 1961).

Industrial Demands Upon the Money Market, 1919-57: A Study in Fund Flow Analysis, by Wilson F. Payne (February, 1961).

American Exports During Business Cycles, 1879-1958, by Ilse Mintz (April, 1961).

Methods for Improving World Transportation Accounts, Applied to 1950-53, by Herman F. Karreman (April, 1961).

Productivity Trends in the United States, by John W. Kendrick (April, 1961).

The Postwar Residential Mortgage Market, by Saul B. Klaman (June, 1961).

Capital in the American Economy: Its Formation and Financing, by Simon Kuznets (June, 1961).

The Growth of Public Expenditures in the United Kingdom, by Alan T. Peacock and Jack Wiseman (July, 1961).

REPORTS NEARING COMPLETION

The Share of Top Wealth-Holders in National Wealth, 1922-56, by Robert J. Lampman.

Industrial Output in the Soviet Union, by G. Warren Nutter.

Long Swings in the Growth of the American Labor Force, by Richard A. Easterlin.

The Money Stock of the United States, 1867-1960, by Milton Friedman and Anna J. Schwartz.

Diversification and Integration in American Industry, by Michael Gort.

Trends in Foreign Trade of the United States Since 1879, by Robert E. Lipsey.

Transportation in the Soviet Union, by Ernest W. Williams, Jr.

The Flow-of-Funds Approach to Social Accounting, by the Conference on Research in Income and Wealth.

The Rate and Direction of Inventive Activity: Economic and Social Factors, by a Special Conference of the Universities-National Bureau Committee for Economic Research and the Committee on Economic Growth of the Social Science Research Council.

Postwar Cycles in Manufacturers' Inventories, by T. M. Stanback, Jr.

The National Wealth of the United States in the Postwar Period, by Raymond W. Goldsmith.

Net Worth Changes and Price Level Changes, by Raymond W. Goldsmith and Robert E. Lipsey.

The National Balance Sheet and the Position of Housing in It, by Raymond W. Goldsmith and Robert E. Lipsey.

The United States Savings Bonds Program in the Postwar Period, by George Hanc.

Tax Treatment of Entrepreneurial Income in the Individual Income Tax, by C. Harry Kahn.

Small Scale Industry in the Soviet Union, by Adam Kaufman.

New Programs. Two new projects were authorized during the year, one dealing with interest rates; the other with the relationship of taxation to economic growth. In addition, the work in the field of international economic relations was significantly expanded to include studies which would focus initially on the position and role of the United States in the world economy.

Universities-National Bureau Committee for Economic Research. The twelfth and thirteenth special conferences under the sponsorship of the Universities-National Bureau Committee for Economic Research were held in the spring of 1960. The twelfth, devoted to "Labor Economics," was held April 22-23 at Princeton University. The thirteenth, devoted to "The Economic and Social Factors Determining the Rate and Direction of Inventive Activity," was held May 12-14 at the University of Minnesota. The Committee on Economic Growth of the Social Science Research Council jointly participated with the Universities-National Bureau Committee in sponsoring this Conference. The proceedings of these conferences are now being edited and prepared for publication.

Directors, Officers, and Staff. At the 1960 annual meeting Crawford H. Greenewalt was elected a Member at Large of the Board of Directors. Oswald W. Knauth and N. I. Stone were elected Directors Emeriti.

Beardsley Ruml, Director at Large since 1936, died on April 18, 1960.

Hal B. Lary was elected a member of the research staff and appointed Associate Director of Research. Others elected members of the research staff are William H. Brown, Jr., Joseph Conard, Robert E. Lipsey, and Jacob Mincer.

Finances. Grants of funds to aid the new projects were received from the Life Insurance Association of America for the study of interest rates; from the Rockefeller Foundation for the program of research in international economics; and from the Rockefeller Brothers Fund for the study of taxation and economic growth.

Grants were received from the National Science Foundation and International Business Machines Corporation to aid the application and use of electronic computers in business cycle and other studies.

These grants and contributions and subscriptions from other sources supplemented greatly the National Bureau's income from the long-term grants from the Ford Foundation and the Rockefeller Foundation, its largest single sources of support.

WILLARD L. THORP

REPORT OF REPRESENTATIVE TO THE AMERICAN
ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

The affiliation of the American Economic Association with the American Association for the Advancement of Science is a symbol of the status of economics in the scientific community. This status seems to me to be lower than it should be. It is reflected, for instance, in the very small membership of Section K of the AAAS. As a result of this small membership, the status of fellow of the AAAS as far as economics and the social sciences are concerned is a distinction for the undistinguished. Very few economists attend the AAAS meetings or subscribe to its journal, *Science*. The fact that the times of meetings of the two societies usually overlap or coincide is perhaps one reason for this lack of interest. It cannot, however, be the only reason. Economists seem to have shown little interest in the economic problems of the scientific community. It is not surprising, therefore, that the scientific community is very little aware of economists or of their possible contributions to its welfare, either intellectually or practically.

In my own view, this is to be deplored. I would like to know whether this view is shared by the members of the Executive Committee. If it is shared, I would like to get suggestions for improving the present position. I would like to suggest, for instance, that this is one among many cogent reasons for moving the meetings of the American Economic Association to the September period, at which time nearly all the other social science associations now meet. This would reserve the present December period for enlarged social science participation in the meetings of the AAAS. It is these meetings which should be a symbol for the whole community of science and which should provide the best forum for integrated discussions.

I would like to get the opinion of the Executive Committee, also, as to the possibilities of a joint campaign for membership in the two associations. Could there, for instance, be some statement by the American Economic Association encouraging its members to join the AAAS?

<div align="right">KENNETH E. BOULDING</div>

REPORT OF REPRESENTATIVES TO THE INTERNATIONAL ECONOMIC ASSOCIATION

During the past two years, the International Economic Association has continued its original activities and has developed a number of new activities.

Since its foundation in 1950, the Association has organized annually: (1) The preparation and publication of a volume of the *International Economic Papers*. Ten volumes have now been issued, as well as a special volume on *Classics in the Theory of Public Finance*. (2) A round table conference of thirty to forty specialists which takes place at the beginning of September each year. The papers and discussions of this conference are subsequently published.

The newer activities, which have now been going on for six years, consist principally of the following: (1) the organization of advanced "refresher courses" in economics in underdeveloped countries where the problem is mainly one of assisting younger economists, and (2) the organization of "regional conferences" where the main objective is the theoretical discussion among senior economists of the economic problems of a given region. The papers presented, as well as a summary record of the discussions at these conferences, are also published.

These new activities were made possible mainly owing to a substantial grant from the Ford Foundation. UNESCO has also played a part in these newer activities by supplying financial and administrative aid for most of the projects.

Consequently, the IEA now each year organizes two or three conferences (round table or refresher courses) and issues two or three volumes—a result which corresponds to the main functions of the Association as defined by the views expressed at the Fourth Council Meeting in Elsinore (1959) when it was agreed that the Association's functions had become twofold: (1) the exchange of ideas between specialists working in particular fields, and (2) the carrying of modern economics to parts of the world that were out of touch.

The continuation of these newer functions has been assured for a further period by a second grant from the Ford Foundation of $175,000 for a five-year period to enable the Association to continue and expand these "missionary" activities in the underdeveloped areas.

Volumes 9 and 10 of the *International Economic Papers* were issued in 1959 and 1960, respectively. As previously, each consists of about 250 pages, published jointly by Macmillan & Co., Ltd. of London and the Macmillan Company of New York. They contain from six to ten translations, prepared for the IEA, of important papers written in a language other than English and which are in general not easily accessible. The Secretary of the Editorial Board is now Hans Liesner, Cambridge.

The last two annual conferences (round table) were held in Elsinore, Denmark (September 2-10, 1959) and in Konstanz, Western Germany (September 2-11, 1960). The subject of the first was "Inflation," where papers were presented by G. Arvidson, P. Baffi, E. Chamberlin, C. Dow, J. Duesenberry, H. S. Ellis, W. R. Gardner, E. Gudin, G. Haberler, Bent Hansen, M. W. Holtrop, Per

Jacobsson, E. James, E. Lindahl, E. Lundberg, A. W. Marget, Hans Moller, F. Neumark, J. Niehans, R. Oyrzanowski, G. Papi, J. Pedersen, J. Rueff, B. Suviranta and R. Triffin. The book containing the papers and a summary record of the debate will be edited by Douglas Hague and published by Macmillan & Co., London.

The subject of the 1960 round table conference was "The Economics of 'Take-off' into Sustained Growth" and the authors of the papers presented were: K. E. Berrill, Mogens Boserup, Otavio Bulhoes, A. K. Cairncross, Paul Cootner, Phyllis Deane, Wolfram Fischer, Alexander Gerschenkron, H. J. Habakkuk, W. G. Hoffmann, S. Kuznets, Harvey Leibenstein, Jean Marczewski, Douglass C. North, W. W. Rostow and Shigeto Tsuru. The book including the papers and a résumé of the discussions is to be edited by W. W. Rostow.

In 1959 and 1960, the following symposia have been published for the IEA after conferences held in previous years: "Stability and Progress in the World Economy," edited by D. C. Hague (the Rome Congress of 1956); "The Economic Consequences of the Size of Nations," edited by E. A. G. Robinson (the Lisbon Round Table of 1957).

A "Refresher Course in Advanced Economics" was organized at the University of the Philippines near Manila from April 12 to 27, 1960, under the direction of Professor Howard S. Ellis, the opportunity being taken of the presence in the Far East of Western economists who had just attended the regional conference at Gamagori (Japan). Professor Amado A. Castro, Director of the Institute of Economic Development and Research of the University, was in charge of the local organization. About fifty participants, mainly Filippinos but including others from Indonesia and Malaya, attended the meetings. Various subjects relating to the problems of economic development in the Far East and Southeast Asia were discussed. Each session was opened by a lecture given by a visiting or local economist.

Regional Conferences. A round table conference on "Economic Development with Special Reference to East Asia" was held in Gamagori (Japan) from April 2-9, 1960. The authors of the papers distributed were J. Adler, K. Berrill, H. B. Chenery, H. S. Ellis, L. Fauvel, M. Kimura, H. Kitamura, P. S. Lokanathan, W. H. Nicholls, K. Ohkawa, S. Okita, E. A. G. Robinson, A. K. Sen, I. Svennilson, T. W. Swan, M. Tachi, C. N. Vakil and S. Yang. The texts of these papers, together with the summary record of the discussions, will be edited by Kenneth Berrill.

A volume containing the papers and the record of the discussions of the first regional conference, held in Rio de Janeiro from August 19-28, 1957, was edited by Howard S. Ellis, assisted by Henry Wallich. It has been published in Spanish under the title, *El Desarrollo Economico y America Latina*, by the Fondo de Cultura Economica (Mexico City). It will appear shortly in English under the title, *Economic Development for Latin America* (Macmillan).

Plans for the immediate future include:

International Economic Papers. The preparation of Volume 11 is under way. The publication of a second specialized volume is being considered. A

fresh start has been made in the preparation of the first of a series of volumes in French, "Textes choisis des economistes etrangers." It is hoped that it will appear in 1962.

The Annual Conference of 1961 (September 1-10) on "Trade Theory in a Developing World" will be held in Switzerland on Lago Maggiore. The Chairman of the Program Committee is Sir Roy Harrod; the members are Professors Maurice Bye, C. P. Kindleberger, K. V. Raj, together with the President and Treasurer of the IEA (Professors Robinson and Neumark).

A Regional Conference on "Economic Development in Africa South of the Sahara," will be held in Addis Ababa in July, 1961. The Chairman of the Program Committee is Professor E. A. G. Robinson; the members are Professors Carlson, Dupriez, Leduc, and Stolper. Economists throughout Africa are greatly interested in this conference, and it is expected to make a contribution to thought regarding African development.

A Refresher Course for English-speaking teachers of economics in Africa will, it is hoped, be held in connection with the 1961 African Regional Conference and will make use of the same resources. The courses will be organized by Professor Robinson. UNESCO is expected to make a financial contribution towards the cost of both these projects.

A Conference on "Labor Productivity" is to be held in Northern Italy (August 24-September 4, 1961). It was proposed in the first place by Professor J. T. Dunlop. Special funds for it have been provided by the Ford Foundation. The program and other details of the conference were decided at a meeting in London on December 18-21, 1960, of the Program Committee appointed by the IEA. This conference will be comparable to the conference organized by UNESCO with technical advice from the IEA which was held at Bursa in Turkey (Easter, 1958). When the USSR first became associated with the activities of UNESCO, they requested the holding of small conferences between Communist and non-Communist economists. The conference on labor productivity this year will fulfill a similar purpose; i.e., the discussion between East and West of questions of common interest. The purpose is to understand each other, not to convert each other. The Program Committee, with Professor J. T. Dunlop acting as Chairman, includes Professors V. P. Dyachenko, G. A. Prudensky (USSR), M. Pohorille, B. Minc (Poland), A. Nove (United Kingdom), and W. Galenson (U.S.A.). The conference will consider three major topics: the meaning of productivity concepts, the measurement of productivity, and the major factors affecting productivity.

An International Congress (1962). It was agreed, at the Fourth meeting of the Council (1959) that a second open Congress similar to the Rome Congress (1956) should be held in 1962, which all members of affiliated associations would be free to attend. The subject of it would be "Economic Development." It would be divided in four sections, dealing respectively with "Growth: its Determinants, its Finance, and the Problems of Inflation," "Techniques and Problems of Development Planning," "Industrialization and Labor Productivity," and "The Stabilization of Primary Producing Economies." It is intended that each section shall have a program chairman and that the selected chairmen should give general addresses to the Congress as a whole

on the topics covered by their respective sections. It is proposed to hold the Congress, if practicable in Vienna.

A Conference for Younger Economists. The Executive Committee agreed to inaugurate a new type of round table: to invite a strictly limited number of younger theoretical economists to discuss some developing topic of technical interest. It is expected to hold the first of these round tables in 1962.

A Refresher Course in Northern Latin America. The Secretariat is exploring with UNESCO the possibilities of holding a refresher course somewhere in northern Latin America in 1962.

The Executive Committee for the period 1959-62 was elected as follows: President: Professor Austin Robinson; Vice-President: Professor Eugenio Gudin; Treasurer: Professor Fritz Neumark; Members: Howard S. Ellis, Emile James, Dimitrios Delivanis, Walter Joehr, Edward Lipinski, Ichiro Nakayama. Professor Lundberg has been co-opted to fill the place left vacant by the death of Professor Lindahl.

The Secretariat is now established at the following address: Faculté de Droit et des Sciences Économiques de Paris, 12 Place du Panthéon, Paris 5eme. The Secretary General is Professor Luc Fauvel and the Administrative Secretary is Miss Mary Crook.

HOWARD S. ELLIS
GOTTFRIED HABERLER

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